

JOURNAL  
OF THE  
AMERICAN ORIENTAL SOCIETY

EDITOR  
W. NORMAN BROWN  
*University of Pennsylvania*

ASSOCIATE EDITORS  
JOHN K. SHRYOCK  
*Philadelphia, Pa.*

E. A. SPEISER  
*University of Pennsylvania*



VOLUME 57

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## BEAD MAKING IN ANCIENT SIND \*

ERNEST MACKAY

MONK'S RISSBOUGH, ENGLAND

BY THE EXCAVATIONS in 1935-36 of the Expedition of the American School of Indic and Iranian Studies and the Museum of Fine Arts, Boston, at Chanhu-daro in the Nawabshah District of Sind, India, remains of the same culture have been revealed as in the ancient Indus cities Mohenjo-daro and Harappa. All three cities were populated by people of this Harappa culture during the period 3600-2500 B. C., and there is no evidence as yet of any previous occupation of these sites.<sup>1</sup>

A few unfinished beads were found during the excavations at Mohenjo-daro. Their number was so small, however, as to suggest that though bead-making was a craft practised in that city it was not carried on to any great extent, unless it were in parts of the city that have not yet been explored.<sup>2</sup>

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\* This article comes to the JOURNAL from the American School of Indic and Iranian Studies and the Museum of Fine Arts, Boston, which jointly conducted a year's excavation at the archaeological site of Chanhu-daro, Sind, during the season of 1935-36. The license to excavate was the first granted by the Government of India under the amended Ancient Monuments Act, which now permits private archaeological excavation in India in accordance with specific rules and under the supervision of the Director General of Archaeology. The American School of Indic and Iranian Studies was planned by a committee appointed by the American Oriental Society in 1927 (see JOURNAL, Vol. 47, p. 358), and then later was fostered by the Committee on Indic and Iranian Studies of the American Council of Learned Societies. It was incorporated in 1934, and this excavation was initiated by the School. The School was fortunate in securing as field director of the excavation Dr. Ernest Mackay, whose long and varied archaeological experience includes six years of work at Mohenjo-daro. The expedition's share of the finds are now in the Museum of Fine Arts, Boston, which owns them permanently. This article is the first detailed scientific study to be published dealing with any of the finds of the excavation; brief general accounts by Dr. Mackay have appeared in the *Bulletin of the Museum of Fine Arts*, October, 1936, and *The Illustrated London News* for November 14 and November 21, 1936.—EDITHS.

<sup>1</sup> The term "Harappa culture" is preferable to "Indus Civilization," which is too comprehensive: other cultures have been and will be found in the ancient Indus valley.

<sup>2</sup> At least three-quarters of the site awaits examination and evidence of many crafts may well be found in the workshops in which they were carried on.



Quite early in the excavation of Chanhu-daro numbers of unfinished beads were unearthed, and later in the season, when we had removed the uppermost strata of the largest mound, we came across not only large numbers of incomplete beads but also the raw material from which they were made, and, still more interesting, the actual stone drills by which they were bored. In addition to providing other interesting material, Chanhu-daro has proved to have been a great centre of bead-making, from which industry it undoubtedly derived a great measure of its prosperity.

Curiously enough, very few finished hard stone beads were found. This can only be explained by the fact that the people of Chanhu-daro had more than once to desert their little city by reason of floods. The river Indus, which is now some twelve miles distant from the site, in former days was only three miles away, and during the actual occupation of the city it may have been nearer still and been a constant menace. The floods that imperilled the city on each occasion left their traces in the subsidence of the buildings; and eventually a more than usually destructive flood led to the complete desertion of the city, which lay derelict some five hundred years or more until its re-occupation by people of another culture, with whom we are not here concerned. The bead-makers, then, were several times forced to leave their city, and they must have taken with them all their finished stock, leaving only unfinished material behind, probably with the idea that they would return later to carry on their craft.<sup>3</sup>

Among the various beads made and worn by the people of the Harappa culture the long barrel-cylinder shape (Pl. I 8, top) was a favorite. Very fine specimens have been unearthed at Mohenjo-daro, some no less than 4.85 in. long and made of the finest translucent carnelian that it was possible to obtain.<sup>4</sup> No beads of this size, even unfinished, have been found at Chanhu-daro, where the longest specimen, which is yellow agate and unbored (Pl. I 8, third from top), is only 3.65 in. long. Nevertheless, most of the beads of this type were of a very respectable length and when finished must have commanded a large price.

It is not my intention to discuss the relationship of these beads

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<sup>3</sup> We have evidence that a considerable interval elapsed before the city was reoccupied each time that it was abandoned.

<sup>4</sup> *Mohenjo-daro and the Indus Civilization*, p. 611; Pl. CLI, b.

with those found in other countries. Suffice it to say that very similar beads, both of agate and carnelian, have been found at Sumerian sites, contemporary in date with those from Chanhu-daro and Mohenjo-daro; but it is still uncertain whether Sumer imported beads of this kind or whether they were made there.<sup>5</sup>

From the great number of unfinished beads in various stages of manufacture that have been found at Chanhu-daro, we now know exactly how the finished products were arrived at. Quantities of the raw material, nodules of agate and carnelian, were found in various parts of the site (Pl. I 2, 5, 6), varying in size but averaging some 3 to 4 in. long. Each nodule had a rough wrinkled skin, of which small pieces had often been removed to show the color inside. But many were sufficiently translucent to reveal the color of the interior if held against a strong light. Often the color was far from uniform; some nodules were a muddy yellow with red or darker yellow cloudy areas, others were clear agate with occasional dark brown patches; and, judging from the reddened surface, certain of them had been roasted to intensify the color of the interior, a practice which is still common in India and other parts of the world.

These nodules of agate and carnelian had all been brought from a distance; no suitable colored stone is procurable in Sind. Most probably they had been brought by sea and then up the river to Chanhu-daro, possibly from the region of Rāmpur in Ahmadabad, or from a site near the modern village of Ratanpur in the Rajpipla State, or, it may be, from localities in Kathiawar where various kinds of agate are still to be found.<sup>6</sup> The two former districts still produce agates and carnelians, which after being treated to improve their colors are sent to Cambay in the Gujarat Division of Bombay, where an extensive bead industry is still carried on.<sup>7</sup> It is more

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<sup>5</sup> I am inclined to the view that these beads were obtained from India, for no unfinished specimens have yet been found in Sumer, though unfinished beads of other types are well known there. It is tempting to think that perhaps Indians actually practised their craft in Sumer as well as exported beads thither.

<sup>6</sup> Sir Edwin Pascoe in *Mohenjo-daro and the Indus Civilization*, p. 481.

<sup>7</sup> For an excellent article in which the various sites from which agates and carnelians have been obtained in historic times in India are discussed, see A. J. Arkell, "Cambay and the Bead Trade," *Antiquary*, Vol. X, pp. 282-303.



than likely that the raw material reached Chanhu-daro from these districts, perhaps through the river port of Broach.

In order to obtain as long a bead as possible, the nodule of agate or carnelian<sup>2</sup> was split along the longer axis to produce a number of slips or rods, square or slightly rectangular in section. At each end every slip has part of the original surface of the nodule from which it was struck, and this sometimes also remains along one side of the slip. To prepare the slips from a nodule by striking alone would have been a very wasteful, as well as difficult task. In actual fact, one or two grooves along the exterior angles of most of them show that the nodules were first sawn longitudinally and then cleaved into sections when the cuts were deep enough (Pl. I 8, bottom: the section is 3.23 in. long). Judging from the rounded bases of these saw-cuts, it was at first thought that a wire saw was used, together with a fine abrasive, emery or the like; indeed, this form of saw was used in Sind until only a short time ago for cutting up agate and similar hard stones. But it seems more likely that a toothless metal plate was used as a saw; it would have the advantage of making a straighter and more level cut, the angles of which would, of course, later become rounded by wear.<sup>3</sup>

After the rough blanks had been separated from the parent stone, they were worked by coarse flaking to a more even shape. The next stage was the removal of the sharp angles by finer flaking (Pl. I 8, second from bottom: 3.87 in. long), which was followed by more careful and minute flaking until the bead was almost, but not quite, round in section (Pl. I 8, third from top). When this stage had been reached, recourse was had to grinding, and the bead was finally shaped by rubbing it to and fro on a piece of coarse sandstone (Pl. II 13).

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<sup>2</sup> Most of the stone was agate. Very few nodules were carnelian, even then the red was far from uniformly distributed. None had the deep uniform translucent red that is seen in the long barrel-cylinder beads of Mohenjo-daro. Possibly this deep red was obtained after the bead had been shaped and bored.

<sup>3</sup> At Cambay, a plate-saw is used for cutting agates and the like, with, of course, the aid of an abrasive. For particulars of this, see Arkell, *op. cit.*, p. 287.

As iron was not then known in Sind, copper saws must have been used; owing to the softness of the metal, they would have been quite as effective with the aid of an abrasive. Bronze would have been no good owing to its greater hardness; though, of course, this alloy was well known and used for other implements.

Quite a number of these bead-stones were found, some of which had evidently had considerable use and others very little. It appears that the grooves in the more used stones were not prepared beforehand with the idea of shaping the bead, but were actually worn by the rubbing. These grooves all show a slight trace of polish, though no abrasive was used with them, for the stone was sufficiently gritty.

Some of these rubbing-stones are shaped like an animal, with what idea we do not know. No. 6 in Pl. II, which is 9.5 in. long and is made of sandstone similar to the ordinary bead-stones, is one of three that were found at Chanhu-daro; others of exactly similar shape have been unearthed at Mohenjo-daro. These animal-shaped stones have mostly been found broken, and the one illustrated had lost its tail which was thick and carried horizontally. Their backs are invariably grooved from side to side by the rubbing of beads across them.<sup>10</sup>

After the bead had been rounded, the rough ends were ground down flat, and before the final polishing the boring was commenced. In rubbing down the bead care had to be taken that its barrel-shape was preserved, and this was done by holding it so that only one-half of its length at a time was in contact with the stone. When this half of the bead had been ground, the other was done. The bead was apparently held in the hand during this process, which did not need particular skill and may have been entrusted to boy or girl labor.

Each end of the bead was then prepared for boring by first roughening its centre so that the drill should not slip. It is not quite certain with what kind of tool this was done, but certain ribbon-flakes terminating in a point (Pl. II 1-3)<sup>11</sup> were, I think, used for this purpose. This preliminary roughening is illustrated in Pl. III 1, which is a micro-photograph of one end of an unfinished long barrel-cylinder bead (diameter 0.3 in.). It will be noticed that the drill had a splintering rather than a true cutting action.

From examination of the unfinished hard-stone beads that were made by the people of the Harappa culture, it was at first thought

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<sup>10</sup> The animal is thought to be a mongoose, but the large eye seems against this identification. These animal bead-stones were at first thought to be hones for household use, but the grooves are too deep and narrow to have been made with metal tools.

<sup>11</sup> From the left these measure 1.53 in., 1.4 in., and 1.5 in. in length. They are light gray and dark brown chert.

that copper or bronze tubular drills were employed to bore the holes. Beads that have been broken in the boring show at the break a slight polished core surrounded by a smooth circular ring. Numbers of examples have been found at Chanhudaro and a few at Mohenjodaro, all with this characteristic pimple in the centre of the break. Pl. III 5, showing this feature, is a micro-photograph of the other end of the bead illustrated in Pl. III 1 (diameter 0.3 in.).

That stone and not metal drills were used in boring the hard-stone beads of Chanhudaro is now proved by a large number of stone drills being found there. Pl. II 5 illustrates a few of these drills, some broken and some perfect, all with the exception of the topmost photographed with the working end downwards. The perfect specimens average 1.5 in. long by 0.1 to 0.12 in. in diameter (Pl. II 5, left-hand bottom corner). The Director of the Geological Survey of India has kindly examined two of these drills for me and reports on them as follows: "Two specimens, described as 'Drills,' consist of chert, containing a little magnetite; the hardness of the specimens is 7. Such specimens do not occur in nature in this rod-like form; they have apparently been worked into shape from material likely to occur in any of the Archaean rocks of India."

The business end of these stone drills is rounded, with a slight depression in the centre. A micro-photograph (Pl. III 8) of the end of one (greatest diameter 0.12 in.) clearly shows the concentric markings formed by an abrasive in its rotation against a hard substance, or, alternatively, the rotation of a hard stone against it. All these drills, whether black or dark brown in color, were made by roughly flaking the stone into a rod-like shape (Pl. II 4: 1.5 in. long), and then grinding them in much the same way as the beads. They are never of the same diameter right through, but narrow slightly just above the working end and then thicken towards the butt, which more often than not has slightly facatted sides, doubtless to prevent its turning in the handle or chuck in which it was fixed. It might not at first appear possible to use so brittle a drill with the hardness of 7 against a material of similar hardness, such as agate or carnelian. By itself the drill would have made little or no impression on these stones, but the use of a fine abrasive with it, such as emery or crushed quartz, would entirely alter matters. Dr. C. H. Desch, Director of the National Physical Laboratory, Teddington, England, to whom I submitted some of these drills for experiment, writes as follows: "I think that the depression at the



end of the drills (see Pl. III 8) is intended to hold the abrasive under the drill and prevent it from escaping. I mounted one of the stone drills in a small Archimedean brace, which I held vertically. The action would be just the same as that of a bow drill. Using 120-mesh emery and water, I found that it took about 20 minutes to drill a depth of a millimetre in one of the rough pieces of carnelian. A small depression must have been made to locate the drill, as on a flat surface the drill wanders around before getting to work. The drilling is certainly quite practicable. Failing emery, possibly even sand may have been used. The wear on the drill is very slight.<sup>12</sup>

The depression spoken of by Dr. Desch as a necessary preliminary for guiding the drill is that already mentioned as occurring on the ends of many of the unbored beads (Pl. III 1), and was probably made with a tool like those seen in Pl. II 1-3. I had early arrived at the opinion also expressed by Dr. Desch that the slight cavity (Pl. III 8) in the points of these drills might serve to hold some of the abrasive; but if this were so, then it is a moot point whether the cavity was purposely hollowed out or was caused by the work that the drill had to perform. There is also the difficulty that the cavity, if it had held abrasive, would hardly have produced the effect in the beads of the slight pimple that is commonly found in those beads that were broken during boring (Pl. III 5). No finished drill has been found without this cavity, and it is reasonable to assume that it was purposely contrived. Its presence led to the supposition that the drills which we first found were unfinished beads, but their likeness to one another in the shallowness of the hole soon changed my views.<sup>13</sup> The slight narrowing of the drill just above the point was doubtless very useful in holding water as well as the abrasive, and thus preventing undue wear of the point.

The implement used for holding the drill was most probably worked with a bow. The bow-drill is in common use at the present day in India both for stone and wood. We must, however, not overlook the possibility that a pump-drill was used, as this tool also is well known in India. But whether the bead revolved or the drill itself is uncertain, nor do we yet know whether the beads were

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<sup>12</sup> Mr. Horace Beck informs me that a specimen of one of these drills has been found at Ur; until he saw the drills from Chanhudaro he had thought it to be an unfinished bead.

drilled in a horizontal or vertical position.<sup>23</sup> I am inclined to think that the bead was horizontal; owing to the difficulty of keeping a very long barrel-cylinder bead in exact alignment with the drill, it was probably inserted in a very deep wooden chuck, and perhaps also cemented in to prevent it from shifting. Even if this were done, the boring might occasionally be out of the straight, owing to faulty bearings.

Every hard stone bead, of whatever size or shape, was bored from both ends so that the drill-holes met in the middle or thereabouts. The holes are never of the same diameter throughout, but definitely narrow from the ends towards the middle owing to a certain amount of play either on the part of the bead or of the drill as the boring proceeded. This play was perhaps not a disadvantage as it permitted the easy supply of water and grinding material, which a very tightly fitting drill would prevent. In most cases the boring was carried out with success; in few of the beads did the holes fail to meet more or less accurately in the centre, though some of the finished specimens from Mohenjo-daro must have been exceedingly difficult to thread.

A very fine abrasive must have been used with the drills, for to the naked eye the bore-holes actually look polished, and in this respect they are far superior to the bore-holes made with diamond drills in India today.<sup>24</sup> In my work at Mohenjo-daro I thought that the holes were polished after the boring was finished, but broken beads from Chanhu-daro clearly show that no other finish to the holes was necessary.

Whereas at Cambay the boring is done last, several specimens show that at Chanhu-daro this process was completed before the bead was ground into its final shape and the traces of flaking removed. This was probably because of the fact that there were so many accidents in the process of drilling owing to unseen fissures in the stone. The beads were then given a final polish and were ready for the market.

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<sup>23</sup> On the Gold Coast, long beads are, or were, set in a vertical position encased in a block of wood while being bored with the aid of a bow-drill. For this information I have to thank Mr. Henry Balfour, who has this particular appliance on exhibit in the University Museum, Oxford.

<sup>24</sup> A. J. Arkell, *op. cit.*, p. 297. Mr. Arkell does not state how the diamond-headed drills are used in boring the modern Cambay bead, but we may assume that the drills are worked with a bow.

It is not yet known how the very long beads of Chanhudaro were polished; but whatever the process employed it was very successful—even the ends of the beads were rounded off so that they should not cut the string. At the present day in India, a large number of beads are put into a leather bag with fine emery and carlinian powder and the bag is pulled to and fro between two men until the desired polish is acquired.<sup>15</sup> This technique may be effective with the shorter varieties of beads, but it would, I imagine, be hardly suitable for very long ones owing to their liability to break. We know from some of the pottery figurines, as well as from actual finds at Mohenjo-daro, that the long barrel-cylinder beads were frequently strung together and worn as girdles. They were, moreover, valued so highly that when broken they were often repaired, or the broken ends were ground down so that they could be used as shorter beads.

A large number of unfinished disc-shaped beads<sup>16</sup> also were unearthed at Chanhudaro. The blanks for these beads were segments of the long slips prepared for the barrel-cylinder beads. Whether square or slightly rectangular in shape (Pl. I 1), these blanks were bored from either side with the aid of chert drills (Pl. II 1-3), the conical holes meeting in a minute hole in the centre of the bead.<sup>17</sup> The bead was then carefully flaked to round it off (Pl. I 4), a process followed by grinding, either by rubbing each bead separately on a stone or by threading a number together and rolling them to and fro on a slab. I am rather doubtful, however, about the latter suggestion; the interiors of the holes of the unfinished beads are very sharp and would tend to cut any thread passed through them (Pl. I 7).<sup>18</sup> Plate III 2 shows a section of one of these beads after it had been roughly bored (diameter 0.58 in.; height 0.29 in.), and No. 3 on the same plate shows the rough-

<sup>15</sup> Arkell, *op. cit.*, p. 207.

<sup>16</sup> Of the Convex Bicone type, No. A. I. c, of Beck's classification in *Archæologia*, Vol. 77, Pls. II, III.

<sup>17</sup> The same type of bead was bored by a similar method at Hierakonpolis, Egypt, in Old Kingdom times; but the flint tools that were used were smaller: Quibell and Green, *Hierakonpolis*, II, pp. 11, 12. Very similar flint tools found by Campbell Thompson at Abu Shahrein in Mesopotamia were used, I think, for the same purpose: *Archæologia*, Vol. 70, p. 110, Pl. IX. I suspect that the microliths found on so many sites in Sumer were used in bead-making.

<sup>18</sup> The illustrations in Pl. I 1, 4, 7 are about three-fourths of natural size.

ness or splintering of the boring (diameter 0.58 in.). What kind of tool was used to give a final polish to the conical holes of this type of bead is not known; it must have been made for the purpose, for the polished holes of the finished beads retain the original biconical shape.

It is not my intention to discuss separately all the varieties of beads of the Harappa culture found at Chanhu-daro, but several of the rarer types may well be mentioned here. Just inside the doorway of a bead-maker's house a mass of beads was found (Pl. II 14) that was possibly once contained in a basket which had long since perished. These beads, which are extraordinarily small and all of the same cylindrical shape, have become concreted together by salt into a mass, which rests on a small copper dish that cannot be separated from it without disturbing the beads. This dish appears to be a scale pan, of which we have found many specimens both at Chanhu-daro and Mohenjo-daro, and it seems likely that these minute beads were sold by weight after being strung. Judging from the floor-level of his workshop the bead-maker had been established there for some time. Another mass of beads of the same type was found in a small pottery jar at another part of the site. Mr. Horace Beck, to whom I have submitted samples, writes as follows: "The smallest bead which I have measured from Chanhu-daro is 0.0268 in. in diameter, which means 37.3 to an inch. It is stated that some specimens from the Harappa site are as small as 0.031 in., but the smallest that I have measured was 0.025 in. The perforation of the Chanhu-daro beads is approximately 0.01 in. An average of the perforations of the six beads measured was 0.0098, the maximum being 0.0107, the minimum 0.0083. After carefully examining a modern watchmaker's drill which is capable of drilling holes 0.01 in. (0.25 mm.) in diameter, it is difficult to believe that either the drill or the bead could have been held by the hand, so I suggest that some form of lathe or jig must have been used."

Mr. Beck continues: "The only stone beads yet discovered which compare with those from Chanhu-daro are those from Harappa mentioned above and a series from Ur. At Harappa great numbers were found, and they vary considerably in size; at Ur also great numbers of similar white beads were found, but they were in no cases as small as the Chanhu-daro beads, and mostly a great deal larger. They are identical with larger beads of the series from



Harappa.<sup>12</sup> The smallest steatite beads from Ur measured 0.04 in. in diameter. The Ur beads were associated with small beads of carnelian and lapis-lazuli and are thought to have formed part of a girdle. No beads of different materials appear to have been associated with the Indian steatite beads.<sup>13</sup> Only one find of steatite beads has been made at Ur, and the carnelian and lapis-lazuli beads show a different technique, so it seems reasonable to suppose that the steatite beads at Ur were imported."

Every one of this type of bead was bored, and, as the illustration (Pl. II 14) shows, they had once been strung together. How such minute beads could be bored puzzles Mr. Beck and myself. Analysis shows that they are made of steatite, but whether from the actual stone or from a compressed steatite powder has not yet been ascertained. Their varying lengths may come from being made as long tubes which were broken or cut up into short segments (Pl. III 6, height 0.58 in., greatest diameter 0.46 in.; III 7, greatest diameter 0.46 in.). I first thought these beads were made of a steatite composition, and were shaped by painting this composition round a smooth fibre or hair, which subsequent firing removed, but the following report by Drs. F. A. Bannister and G. F. Claringbull, of the Mineral Department of the British Museum (Natural History), leaves room for doubt: "The beads submitted to us are small hollow cylinders which vary in external diameter from  $\frac{1}{2}$  to 1 mm. and in length from  $\frac{1}{2}$  to  $1\frac{1}{2}$  mm. When freed from loosely adherent soil they are pure white and very hard. Nearly every bead is furrowed along its length spirally, always in a clockwise direction. It is not uncommon to find two beads joined together with the spiral furrowing running continuously and in perfect alignment from one to the other. The two beads are connected by a thin layer of friable soil which can be readily removed with a needle. We have carefully examined alternative views of how these tiny beads were made, but so far have been unable to reconcile all our observations with any one theory. The physical and chemical data are conclusive, however, as to the material from which these beads were made. Their specific gravity is 2.85 and the refractive index of crushed fragments is 1.60. The beads can only be fused in the electric arc, and then

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<sup>12</sup> Report on Harappa Beads, 1934, but not yet published, by the Indian Archaeological Survey (H. B.).

<sup>13</sup> Correct for the beads from Chanhu-daro (H. M.).

yield glassy spheres with a lower specific gravity 2.72. Magnesium and silicon but no other elements could be detected with the quartz spectograph, and X-ray powder photographs show that the beads are steatite that has been completely dehydrated and heated to a temperature exceeding 1200° C. X-ray photographs have also revealed that much larger beads from the same locality (India) and Egyptian scarabs also consisting of baked steatite have only reached temperatures up to 1000° C.<sup>19</sup>

This report seems to indicate that the beads were in fact made as long tubes, and the spiral furrowing referred to suggests that these tubes were shaped by squeezing a composition through an aperture; though how tubes rather than solid rods were produced it is difficult to see. Where two beads were found almost united and with their markings continuous, it seems that they must have passed after one another through the same apparatus.

Unfortunately, it is not possible to ascertain whether these beads were made of a paste or cut from the solid stone, for Dr. Bannister tells me that after the very high temperature to which these beads had been subjected anciently, all evidence of their being made of a composition of steatite would have been lost. At any rate these beads show craftsmanship of the highest order combined with extraordinarily good eyesight.<sup>21</sup>

A type of bead that is very common in all the cities of the Harappa culture is the wafer bead,<sup>22</sup> which varies greatly in size and also in the magnitude of the hole that perforates it. The larger beads of this type were generally made of steatite, which was shaped as a long rod that was subsequently cut up into thin segments, apparently by means of a saw. Most of these beads now show no traces of glaze or color.

Certain unfinished beads of this type deserve especial mention on account of their small size. These are thin, flat plates of dark brown steatite, irregular in shape and averaging 0.1 of an inch across. Each of these little unfinished beads is perforated by a minute hole, the diameter of which, according to Mr. Beck, corresponds very closely to the perforations in the tiny tubular beads

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<sup>19</sup> These particular beads can be regarded as finished. Though the majority are now white, Beck has in some detected traces of a broken-down silicate glaze, probably originally blue.

<sup>22</sup> Classified by Mr. Beck as "Disc-heads," A. 2. h., *Archæologia*, Vol. 77, Pls. II, III.

already referred to. A specimen bead (greatest width 0.15 in.) is shown in Pl. III 4. Very noticeable in the illustration is the irregularity of the hole, which looks to me to have been made by a fine metal point twirled between the fingers—an easy process with a soft stone like steatite.

Mr. Beck states that "these plates are either unfinished beads in the process of manufacture, or beads that have for some reason been discarded when only half made. The diameter of the holes corresponds very closely to the perforations in the (tubular) beads. The thickness of the plates varies from 0.012 to 0.02 in. in the specimens measured. It is an interesting fact that they are made of steatite which does not appear to have been treated by burning."

These unfinished beads were apparently made by first cutting up steatite into thin plates which were rubbed down to the requisite thickness and then cut into blanks approximating in area to the size of the beads required. It seems certain that the original plates were thus broken up before the process of boring, for otherwise there would have been a tendency for breakage to occur *through* the holes, instead of irregularly around them. The final rounding off of these beads could have been effected by stringing them tightly on a fibre or hair, and rubbing the uneven rod thus formed smooth all round. This was very likely done after the perforated blanks had been subjected to heat to harden them, for steatite heated at 900 C. for the space of one hour can be taken from 1 to 7 of Moh's scale of hardness, and is, moreover, whitened in the process.<sup>23</sup>

Mr. Beck is inclined to think that these little plates are the beginnings of the minute tubular beads already described. With this, however, I am not inclined to agree. For one thing, the thickness of the perforated plates is smaller, though certainly not by much, than the lengths of the tubular beads. I cannot but think that it was intended to make wafer beads from these plates; we have, in fact, unearthed at Chanhu-daro small finished waferbeads, ranging in size from 0.08 to 0.1 in. in diameter, white in color and pierced with a minute hole, that seem to have been made in this way.

Fifteen specimens of the decorated type of carnelian bead have been found at Chanhu-daro, some of which are illustrated in Pl. II 7-12. Several were broken or defective beads, and it is certain

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<sup>23</sup> For particulars of this treatment, see F. A. Bannister and H. J. Plenderleith in the *Journal of Egyptian Archaeology*, Vol. XXII, pp. 3-6.

that beads of this technique were made in some, at least, of the bead-factories of the city. The design on No. 7, which is a perfect specimen, 0.6 in. long, and, moreover, a darker red than is usually found in this type of bead, is unknown at Mohenjo-daro; but beads of similar design, though different in shape, have been unearthed at several sites in Sumer,<sup>24</sup> where they have been dated to approximately the same period. Bead No. 9 is of particular interest as its design is in black on a whitened ground. Only one specimen of this particular technique, as yet unpublished, has been found at Mohenjo-daro, and another was found by Sir Leonard Woolley loose in the soil of the Sargonic stratum at Ur.<sup>25</sup> The design on this bead which is incomplete and now measures 0.74 in. long by 0.58 in. wide by 0.17 in. thick, is very similar to that on certain beads found at Ur and two at Mohenjo-daro.<sup>26</sup>

The design on No. 10 in Pl. II varies very little from one seen on beads from Ur,<sup>27</sup> and the patterns on Nos. 8, 11, 12, are quite common on decorated carnelian beads from Tell Asmar, Kish, and Ur in Mesopotamia, and Mohenjo-daro and Harappa in India. The great similarity of the patterns on these widely scattered beads strongly suggests that they originated in one centre. It is also significant that in most cases their shapes are alike, and that they occur in levels contemporary with one another. It was only a short time ago that the manufacture of decorated carnelian beads ceased in India, and I feel convinced that it was that country that first exported them, early in the third millennium B. C., and that afterwards the craft was practised in several countries.<sup>28</sup>

I have before remarked that very few unfinished hard stone beads have been found at Chanhu-daro; and carnelian itself is very rare even among the large number of nodules that formed the raw material from which the beads were made. A light or deeper

<sup>24</sup> Frankfort, *Iraq Excavations of the Oriental Institute*, Chicago, 1932-33, Fig. 29; Woolley, *Royal Cemetery*, Pl. 133.

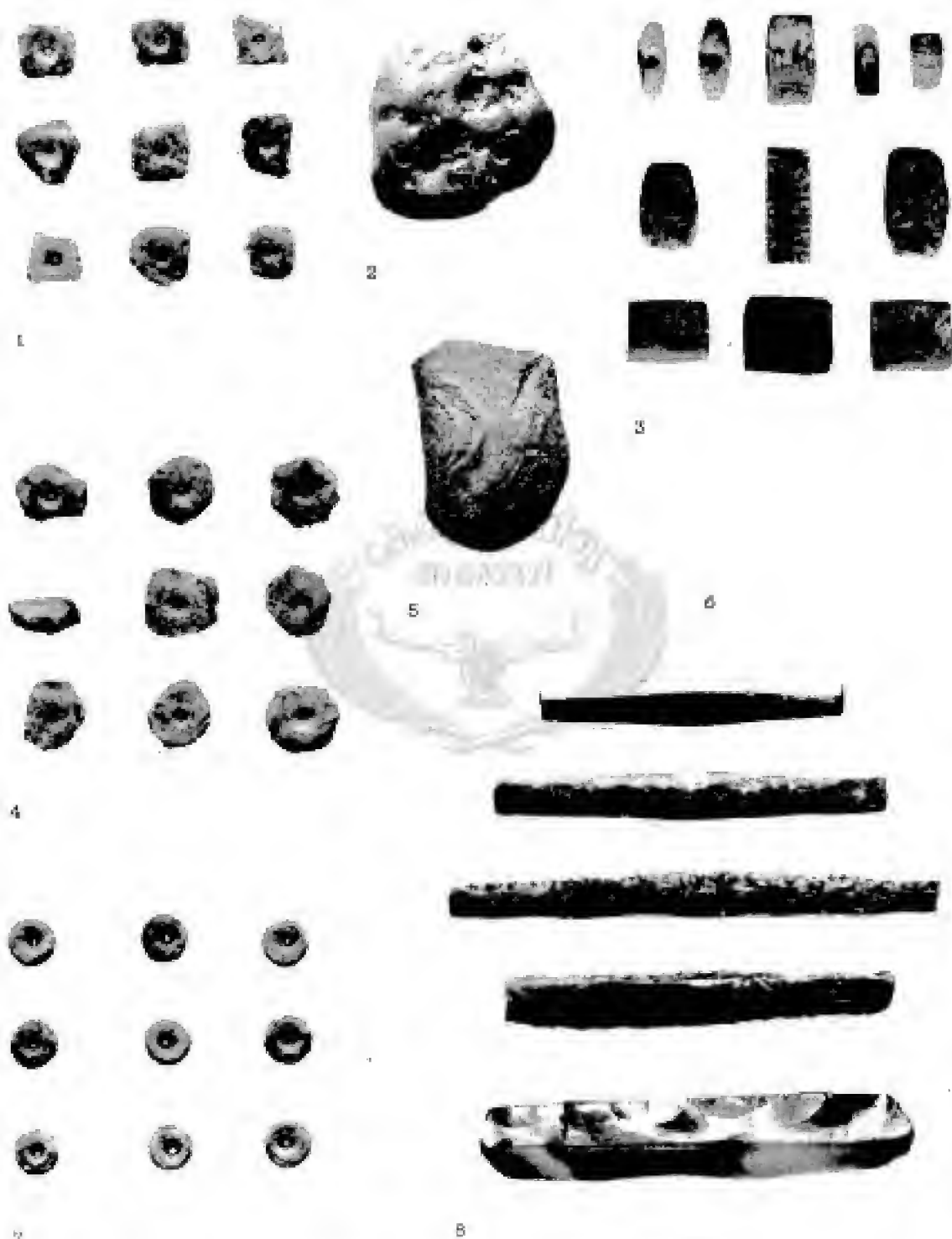
<sup>25</sup> *Antiquaries Journal*, Vol. XIV, p. 550. This technique is also seen in beads from Taxila. Specimens of it have been unearthed at Tell Asmar and in Baluchistan, for particulars of which see Beck, *Antiquaries Journal*, Vol. XIII, pp. 384-398.

<sup>26</sup> *Mohenjo-daro and the Indus Civilization*, Pl. CXLVI, 46. The second specimen will be published shortly.

<sup>27</sup> Woolley, *Royal Cemetery*, Pl. 133.

<sup>28</sup> They have not yet been found in Egypt, but specimens made at a later date come from as far west as Russia.

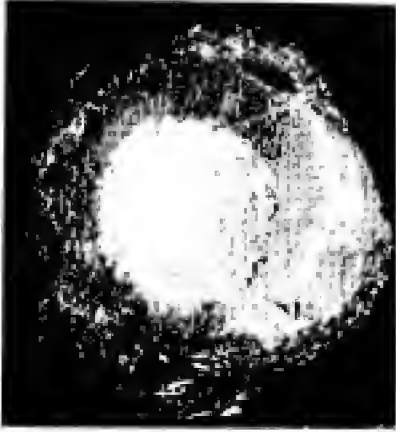




Unpolished Beads from Chander-daro, Sind



Beads and Bead-Drifts from Chander-daro, Sind



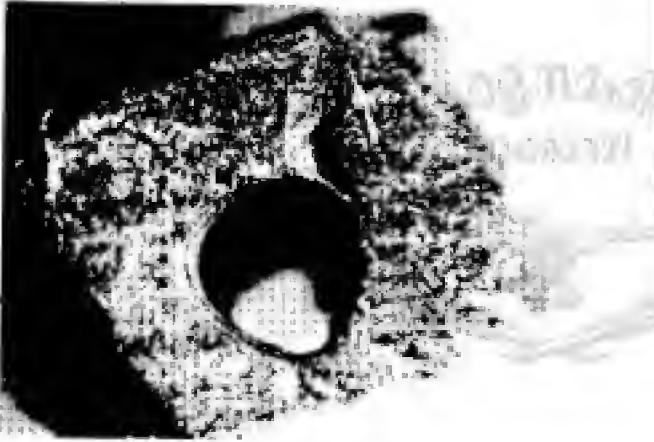
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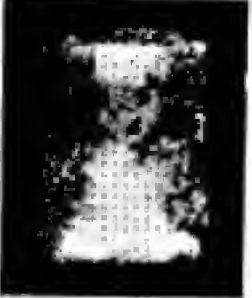
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3



4



5



6

Micro-photographs of heads from *Chondrostoma*, Sted

colored yellow agate, sometimes very cloudy, was the material that was commonly used at Chanhu-daro: but it well known that the colors of agates may be changed by heat, and it is possible that many of the beads, especially the long barrel-cylinder type, were artificially turned red just before, or even after, they received their final polish.<sup>20</sup> Only one bead of the barrel-cylinder type has been found at Chanhu-daro of the uniform deep red so common at Mohenjo-daro and evidently greatly admired. Another bead was yellow at one end, red at the other and dark brown in the centre.

Though many unfinished hard stone beads have been found in Egypt, a country which was certainly a great centre of bead-making, and which from very early times worked more varieties of stone than any other land, it is surprising that except for the rough flint drills reported from Hierakonpolis, nothing approaching the finely finished stone drills lately found at Chanhu-daro seems to have been used. Similar drills may, however, yet be reported from Egypt; it is quite possible, indeed, that they have already been found and identified as something else.<sup>21</sup> Beck now reports one specimen from Ur; and though I have not yet seen them, Dr. Campbell Thompson has drawn my attention to certain obsidian rods which he found at Abu Shahrein, which from their appearance could well have been used as drills, although they differ somewhat in shape from those from India.<sup>22</sup>

In conclusion, I should add that hard stone beads of several other shapes than those mentioned above were made at Chanhu-daro (Pl. I 3). The technique of their manufacture, however, presents no features other than those already described in this review of the bead-maker's craft in ancient Sind.

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<sup>20</sup> It is usual at the present day to alter the color of agate by roasting it in its nodular form at the place where it is mined. Agate can also be converted into carnelian by boiling the stone in a solution of iron: Arkell, "Carbony and the Bead Trade," *Antiquity*, 1936, p. 302.

<sup>21</sup> I would suggest koldatichs.

<sup>22</sup> *Archæologia*, Vol. 70, Pl. IX, Row 8, pp. 120. I have not been able to examine these rods, owing to alterations in the British Museum.



# THE AORIST IN BUDDHIST HYBRID SANSKRIT

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§ 1. THIS is a preliminary draft of a chapter in the Grammar of Buddhist Hybrid Sanskrit.<sup>1</sup> It is based almost wholly on the *Saddharmapundarikā* and *Lalitavistara*, supplemented by a few stray observations from other texts. Most of the materials are taken from the verse parts of those two works, since the prose, at least as presented in the editions, yields little.<sup>2</sup> All forms quoted in this

<sup>1</sup> For general orientation see *BSOS* 6. 501-16; for a similar preliminary account of nouns of the *a*-declension see *HJAS* 1. 65-83. For an account of the metrical peculiarities of the verses of SP see *KSCV* 39-46 (but this article, no proof of which was sent to me before printing, contains some errors and many misprints, some so serious as to result in unintelligibility).—The following abbreviations are used in this article (it is hoped that others will be self-explanatory):

AMg = *Ardhamāgadhī*

Ap = *Apabhraṃśa*

BSkt = Buddhist (Hybrid) Sanskrit

BSOS = *Bulletin of the School of Oriental Studies*, London

Calc. = the Calcutta (*Bibliotheca Indica*) edition of LV.

Geiger = Wilhelm Geiger, *Pali* (Strassburg, 1910)

HJAS = *Harvard Journal of Asiatic Studies*

Hoernle = A. F. R. Hoernle, *Manuscript Remains of Buddhist Literature found in Eastern Turkestan* (Oxford, 1916). Contains editions, by Lüders and Thomas, of several fragments of Kashgar mss. of SP.

KSCV = *The Kuppaswami Śāstri Commemoration Volume* (1936), 39-46 (see above).

LaVP = La Vallée Poussin, *JRAS* 1911, 1967-77 (edition of another fragment of a Kashgar ms. of SP).

LV = *Lalitavistara* (ed. Lefmann unless otherwise specified by "Calc.").  
m. c. = *metri causa*

Mhv. = *Mahāvastu* (ed. Senart).

O = readings of mss. of the Kashgar recension of SP quoted by this designation in the critical notes to Kern and Nanjio's edition.

Pischel = R. Pischel, *Grammatik der Prakrit-Sprachen* (Strassburg, 1900).

SP = *Saddharmapundarikā* (ed. Kern and Nanjio unless otherwise specified by "WT").

WT = Wogihara and Tsuchida's edition of SP (Tokyo, 1934-5).

<sup>2</sup> Perhaps if we knew its original form it would yield more; cf. *BSOS* 6. 507. Our printed texts of SP and LV are based mainly (in the case of

article are taken from verses, except those specifically identified as prose.

§ 2. *Metrical license.* For the purpose of this paper (see *KSCV* for details) it will suffice to recall that in verses any short vowel in an open syllable may be lengthened, and any long vowel shortened, metri causa; and that all final syllables ending in a vowel are open, since consonant clusters at the beginning of a word (the regularly written in Sanskrit fashion) were always pronounced Prakritically, as single consonants. Hence e.g. the Prakritic aorist ending *ī* (Skt. *ī*) is constantly shortened to *i* when the meter requires a short syllable; and the semi-Prakritic ending *īṣu* (3 pl.) may appear as *īṣ*, *īṣu*, or *īṣā* according to metrical requirements.\*

LV wholly) on Nepalese tradition. What we know of the Central Asian or "Kashgar" recension of SP suggests that its prose was much more Prakritic, and presumably closer to the original text. It is regrettable that we have no complete edition of it. Kern and Nanjio quote some readings of fragmentary Kashgar mss. by the collective symbol "O"; other fragments have been published by Lavi<sup>2</sup> and by Lüders and Thomas in Hoernle (see note 1). We shall see that these fragments present some Prakritic aorist forms where the printed text of SP has regular Sanskrit forms.

\* Altho the latter would be equally satisfied by *u*, before an initial consonant, of the Sanskrit ending  $\{i\}\text{-}u\bar{a}$  (or its sandhi equivalent), instead of  $\{i\}\text{-}a$ , for  $\{i\}\text{-}u$ , this very rarely occurs in forms that are otherwise Prakritic or semi-Prakritic.—The freedom with which vowels are lengthened and shortened according to metrical requirements sometimes leaves us in doubt as to whether a long or a short vowel was normal in a given form; that is, whether the one or the other is due to metrical necessities. If either is clearly established in prose, this of course settles it; but frequently the prose has neither. In that case we must try to find the form in metrically indifferent positions. Such positions include the first half of anuṣṭubh (śloka) pādas, and in SP the final syllables of all pādas. On this basis we can say with some confidence that the common 3 sg. aorist form of the Skt. *iṣ*-type (*dehāṣ* etc.) had *i*, for this is often found at the end of lines in SP, and short *i* seems almost never to occur there (I have noted only *abḥapayāṣi* SP 101.8; to be emended?), nor in the first half of anuṣṭubh pādas, nor in prose (unless we accept *upāhāṣapayāṣi*, the v. l. of O in SP 100.8, prose, for ed. *sapathāpayaṣi*; in SP 100.16 *āṣi* = Skt. *āṣi* [which formally belongs to the aorists in our language, § 22] is printed at the end of a line of verse but we should doubtless adopt the v. l. *āṣi*). On the other hand, and quite surprisingly, the *s*-aorist *abḥāṣi* (root *bḥā*) seems to have that form when the meter is indifferent. The rare *abḥāṣi* occurs only in positions where meter requires a long; SP 343.11; LV 104.3 (at the end of a line but in a meter requiring a long final and otherwise

§ 3. *Third singular forms generalized.* As remarked in *BOS* 8. 504, any third-singular verb form is freely used in our language with subjects of other persons and numbers: most commonly third plural, but not infrequently first and second singular. (First and second plural subjects are rare. Dual forms of both noun and verb, while not unknown, are more often, in the verses, replaced by plurals, as in Pali and Prakrit.) To some extent this phenomenon is known to Prakrit. In the optative it is general Prakrit (Pischel § 469). In AMg. (the only Prakrit treated in Pischel which has the aorist) it is also common in aorist forms (ib. §§ 515-7). Our dialect carries it much further, to all verb forms; but so far as the aorists are concerned it does not differ from AMg. usage. In Pali the usage seems to be recorded only of *atthi* = Skt. *asti* (Geiger § 141).

§ 4. *Dialect relations.* The aorist is extinct in most Prakrits and in Ap.; of all the dialects treated by Pischel it is really alive only in AMg. It is however also fully alive in Pali. In general our language shows great resemblance in grammar to AMg., and almost none to Pali. In its aorist forms this is less strikingly true; there are points on which it resembles Pali more than AMg. However, it also differs from both at times. Indeed it is becoming increasingly clear that the protocanonical Prakrit, which underlies Hybrid Sanskrit, was a quite definite dialect standing on its own feet, and independent of any Prakrit known to the grammarians (and a *fortiori* of Pali).

§ 5. *Augment.* In the verses the augment is omitted in the majority of forms. Even the prose contains unaugmented preterites, some of which would otherwise be regular Sanskrit forms.

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showing lengthening there m. c.) and 167. 15 (at the end of a line of another meter like the preceding, cf. *sukāśa* 168. 17 with a m. c. at the end of a pāda). The *abāṣṭ* occurs in prose according to the Kashgar ms. of Thomas (Hoernle 133 f.) six times where the edition (SP 327. 2, 5, 7, 328. 1, 3, 5) has *abāṣṭ* or *abāṣṭa*, *abāṣṭi* (without final *i*) cannot be proved to occur in prose (to be sure in two out of the above six cases Thomas' ms. reads *abāṣṭ*, but since in the same context the other four read *\*ṣṭ* we must probably follow Thomas in regarding *\*ṣṭ* as a mere corruption).—In LV, with its much more complicated meters, occurrence at the end of a pāda does not have the same significance as in SP. Many of the LV meters require a long final syllable and often show metrically lengthened vowels in that position, as noted just above.

Thus *prati-jñāsiṃ* LV 418. 15, 19, which is the more surprising because it occurs in a very ancient canonical prose passage found in almost identical language in Pali (Mahāvagga 1. 6. 37, 38); and in the Pali form the verb (*paccasāḍasi*) is augmented! Which may, possibly, indicate that our text has kept an original augmentless form which even Pali provided with an augment in its recension. In general, to be sure, our prose shows few augmentless forms in the editions. But probably the original showed more. For the Kashgar recension has them at times where the editions, with the Nepalese mss., have other forms. Cf. *udgṛhṣiṣu dhārayiṣu paryāpuriṣu*, v.l. of O at SP 181. 9, and *samprakampayṣu* Lüders (Hoernle 152) for *samprakampitā* SP 270. 12 (others § 20, below). On the other hand an augmented aorist is used in a *mā* clause, where Sanskrit regularly omits the augment: (*mā...*) *akārṣaḥ* LV 89. 10 (prose). And in the verses occur forms which are un-Sanskritic and nevertheless have the augment; especially from the roots *vac* and *drś* (*avacā*, *adrśā*, etc., to some extent paralleled in Pali), but also from other roots.

### 1. Descendants of the Sanskrit *iṣ*-aorist

§ 6. *Stem form and normal endings.* From the *iṣ*-aorist of Sanskrit is derived the commonest aorist type of our language, which seems to have been indefinitely productive in protocanonical Prakrit. It generally uses the ending *i* (for which *ī* occurs as a substitute, but almost exclusively m.c.), originally 3 sg. (Skt. *it*), but here generalized for all persons and numbers. Fairly common also are distinctively 3 pl. forms corresponding to Skt. *iṣuḥ* (here *iṣu*, *iṣsu*, etc.; details below). Other endings occur rarely in the verses.—The ending *it* is rare in Prakritic forms; *śradddadhi* SP 113. 10, and in prose *prābhañjī* SP 159. 1, based on the BSkt. and Pali present *-bhañjati* (e.g. LV 175. 17).

§ 7. All these endings are regularly added not to the root, but to the theme of the present tense, minus the thematic vowel (*a*). Such forms are extremely common in the verses, tho rare in prose. As is implied by this formulation, I believe that these Prakritic aorists are always based on a thematic present.\* The same condi-

\* For *chīti* "out" LV 145. 22, which can only be a 3 sg. aorist to the root *chid*, I have not yet found a parallel present \**chīneti*. But the exception may be said to prove the rule. At least it is, as far as I can see,

tion holds in Pali and AMg., tho the definite relationship between *iś-ariśt* and *a-present*, which I believe I have discovered here and which I think probably holds good in Pali and AMg. also, seems not to have been formulated. Examples: *paśyisu* SP 16. 5 (*paśiyati*).—*caṅkramat* LV 363. 16 (*caṅkramatas*, 3 dual pres., SP 460. 2, prose).—*kṣipt* LV 92. 16 (*kṣipati*).—*adhyesi* LV 416. 3 (*adhyeṣati* "applies for instruction to," common in BSkt., = Pali *ajjhesati*).—*gacchisu* LV 74. 8 (*gacchati*).—*jahat* LV 194. 17 (*jahati* = *jahati*).—*avacā* LV 370. 14 (cf. *vaced* 3 sg. opt. SP 258. 4, and Pali *avacāṇ* etc.).—*paribhūñjī* LV 387. 7 (*bhūñjati* "U + " in Whitney, *Roots*; frequent in BSkt.).—*śrupi* "heard" SP 90. 4 (*śrups*[t], opt., SP 252. 6; *śrupā*, m. c. for *śrupa*, 2 sg. impr., LV 94. 12; *śrupanti* LV 233. 10, etc.).—*nīrmini* LV 219. 19 (*abhinirmīṣeyāṇ* 1 sg. opt., SP 196. 7, 9; to Skt. *-minoti*, altered to *-mīnati*).—*kureisu* SP 10. 6 (*kureati*, frequent for Skt. *karati*, e. g. SP 28. 3, 125. 14; cf. *BSOS* 8. 515). And many others. In prose (ending *it*), *prābhūñjit*, see § 6.

§ 8. From passive stems (which to be sure, in BSkt. as in Pkt., are quite as apt to have active endings and to be treated in all

impossible to derive *chīni* from any other (that is from any heretofore recorded) form of the root or present stem, Sanskrit or Prakrit. The form actually compels us to assume a \**chīnati*, which may yet turn up somewhere. This would be a blend of Skt. *chīnati* with the regular Pali-Prakrit (and BSkt.) present *chīnāsi* (Pkt. *chīnāsi*). Similarly *leṭṭi* is to be read at LV 197. 1 with v. l. for *leṭṭi*, and is to be connected with Pali *leṭṭati* (blend of Skt. *likati* and *leṭṭi*).—From *dyā* "see" we find *ariśt* *adāṭṭi*, *ḍrāt*, *ḍrāt*, *adāṭṭi* and \**ḍi* (sometimes with Prakritic *a* for *i*; the augmented forms are written with either single or double *d*, but always pronounced with *-dd-*, as the meter proves). Here again we find, to be sure, no present \**dyatāṭṭi*. But we must assume that the type started from the old thematic *ariśt* *adyāṭṭi*, which from the Prakrit standpoint would be felt as an imperfect to a thematic present stem *dyā-*. This clearly underlies Pali *addhaṇṇ*, *addhaṇ* or \**ad* (Geiger, § 162. 3), which has modified it only by doubling the *d* in imitation of the type *addabāṭṭi* (Skt. *adrakṣāt*). Our language has this same modification, and in addition has replaced the old thematic endings by those which had come to be felt as appropriate to the *ariśt*. I find a confirmation of this theory in the 3 pl. form *adras* (for *adyāsu*, pronounced *addyāsu*) LV 27. 22, for Skt. \**adyāṇ* = *adyāṇ*, cf. Pali *addhaṇṇ* (in Pali all 3 pl. *ariśt* forms end in *-ṇ*). See § 32, and for the further adaptation of the same original to *a(d)ḍrāṭṭi* etc. see below, § 28.—In another place I shall shortly show that thematic present stems were used as bases for the whole verb inflection in the protocanonical Prakrit.



respects like actives): *vādyāṇu* "were made to sound" LV 194. 2 (*vādyate*, pass. to *vādayati*).—*drśyāṇu* "were seen" LV 74. 12 (*drśyate*).—*vādhyā* LV 223. 6 (to *bādhyaṭe*, pass. of *bādā*).

§ 9. Particularly common are such forms from stems in *-aya-* (causative or denominative). A few examples: *daśayā* SP 57. 14, *prakāśayā* SP 29. 10, *nāmayā* LV 74. 2, *śamayā* (for *śam<sup>c</sup>*) LV 386. 20. These *-aya-* bases sometimes drop the syllable *ya*, in aorists as well as in other forms (including presents): *śuāpi* LV 271. 12 (aor. to *śuāpayati*).

§ 10. But when this *aya* is Prakritically contracted to *e* (which occurs very often), it is no longer possible to form *this* type of aorist from the contracted stem in *e*; only the *s*-aorist type can be formed from a stem ending in a vowel (except the thematic *a* which is dropped). Hence *śhapesā* from *śhāpayati*, LV 160. 3, etc. (see below § 24).

§ 11. List of forms in *i* or *ī*. Except as stated above in note 3, they occur only in verses (but note *prābhāṣijit* in prose, § 6, and plural forms, §§ 18, 20), and the ending short *i* is found only (or almost only) when the meter requires a short syllable; when the meter is indifferent or requires a long, *ī* is used. They are used for all persons and numbers, the distinctive forms for other persons and numbers also occur, especially for the 3 pl. For the 1 sg. there is one doubtful occurrence of the ending *ya*, formed on the analogy of Skt. *iz*, *it* (cf. Pali *im*): *darśayā* SP 90. 3 (but several mss. read *i* or *ī* without *ya*).

§ 12. Other 1 sg. cases: in SP, *prayojayā* 90. 5; *addaśī* 9. 3 (so emended in ed. for the metrically impossible *addarāśī* of the mss., confirmed by *addaśī* 3 sg. LV 194. 12), *bhayaṇi* 56. 6, *saṃkṣobhā* 56. 8, *daśayā* 57. 14. In LV, *pranīpātī* 231. 19 (? or for *\*pātī*, 1 sg. pres. mid.).

§ 13. 2 sg. (no case noted in SP), in LV, *uddharī* 195. 12, *sampāśyī* 195. 20, *kṛedayī* 221. 8; and (with *i*) *labhī* 53. 4, 166. 14, *janī* 91. 17 (*mā kṛu janī vīśādu* "do not produce despair [become despondent]"), *tyajī* 165. 9, 16, 166. 16, 167. 2, 7, 168. 9, *paricuri* . . . *dravī* 165. 18 (? perhaps gerunds), *upakṣī* 172. 18, *bhāṣī* 235. 9 (from *bhāṣ*, with *a* for *ā* m. c., and *s* for *ṣ* as very often), *kṣatī* 362. 11 (v. l. *śkṣatī*; "and thou hast not stumbled . . ."), *bhīndī* 370. 22.

§ 14. 3 sg.: in SP, *anubhāsayi* 9. 4, *prakāsayi* 23. 10, 193. 4, *dhṛyāyī* 23. 12, *ārāgayi* (v. l. *ārādḥayī*) 27. 12, 384. 8, *cintayī* 99. 3 (read with 0), *paripācayī* 204. 13, *deśayī* 292. 12; and (with ī) *adharayī* 25. 10, *darśayī* 84. 2, 193. 6, *cintayī* 87. 5, *ukrami* (for *o-krami*, from *ava-krami*) 25. 8, *saṃśodhayī* 114. 1, *cavi* 258. 10 (from Prakritic *astati*; read *ananyapakarmāpvi daśadāśāu*; wrongly emended in WT). In Lohkavātara Sūtra, *alepi* 23. 14. In LV, *māmayī* 74. 2, *gacchī* 31. 11, *nīkramī* 92. 12, *avaci* 109. 16, 165. 14, 199. 11, 304. 3, *jahi* 134. 17, *nīkari* 179. 5, 182. 19 (read *śabd' evarūpas tur' nīś'*), (a)*varlayī* 220. 22, *ālepi* 221. 15, *virikhi* 230. 4, *harṣi* 232. 16, *nīrodhayī* 236. 5, *rapi* 236. 19, *arukhi* 236. 15, *viuati* 236. 17, *vraji* 236. 18, *nigati* 232. 3, *saṃpratigrāhī* 316. 8, *saṃprapaśyī* 341. 12, *osari*[r] 357. 3 (read "śaḥsraṃś c' osari[r] ātma°"; the form is 3 sg. and the final r, if it is to be adopted, must be considered a sandhi-consonant or "Hiatus-bridger"), *calīkramī* 368. 16, *kṣipi* 387. 7, *vīkramī* 412. 1; and (with ī) *calī* 92. 3, *mañci* 92. 20, *gacchi* 133. 13, 240. 21, 421. 13, *dhṛyāyī* 133. 21, *amci* 135. 1, 329. 7, 370. 14, *prakṣi* 135. 8, 241. 11, *prajāni* 151. 15, *chīni* 165. 22, *sravi* 166. 2, *bhāni* 166. 17, 20, 233. 7, *addaśi* 194. 12, 22, *adrāi* (pron. *add'*) 194. 16, 197. 15, 17, *āśi* 230. 4, *kari* 200. 9, *codayī* 220. 1, *nīrmāni* 219. 19, *asañiri* 230. 12, *pratibhāni* 231. 21, 232. 3, 10, *upagami* 243. 12, *snāpi* 271. 12, *vraji* 299. 11, *abhīcargi* 301. 6, *saṃayī* (*saṃ'*) 366. 20, *paribhāṣi* 357. 7, *vyāhāri* 393. 12.—In LV 299. 12 *anubuddhi* (no v. l.) is certainly an aorist connected with the root *buddh*; similarly *buddhīṣṭu* LV 230. 12 (§ 18 below), all mss. also -*ddh*-. With these are to be compared the gerunds *buddhīte* LV 163. 10, 164. 12, *buddhīte* LV 231. 19, 355. 15; the pple. *buddhita-* (so mss., emended by Senart m. c. to *budhita-*, and doubtless so pronounced) Mhv. i. 209. 23, and—most important—the present optative *buddhema* LV 361. 10 (with acc. *sarvajñāṇam*; so at least Lefmann reads with best mss.; v. l. of inferior mss. with Calc. *budhyema*). The aorists and the gerunds and pple. all seem to imply a present *buddhati*, the stem of which actually occurs in *buddhema*. At first I inclined to emend all these forms to *budhy-* instead of *buddh-*, connecting them with the stem of *budhyate*; cf. the future *budhyisyate* SP 243. 14 (where however two mss. read *buddhisyate*, so, with dental s). I am still not sure that this is not the true solution, the *buddh-* forms being either corruptions or orthographic or phonetic variants of *budhy-* forms. Since however the -*ddh*- forms occur rather persistently, I am now inclined to

accept a present *buddhañi*, a sort of denominative to the stem *buddha-*. Whether this or the regular form *buddha-ñi* be accepted as basis, in either case we have aorists (and gerunds, futures, participles) based on a thematic stem.

§ 15. 1 pl.: *pranīpati* LV 363.10 (? Foucaux as 3 pl.; cf. parallel *vande* in next two verses). I have noticed no 2 pl. case.

§ 16. 3 pl.: in SP, *nīcārdhayañi* 131.1, *nispādayi* 131.2, *upasaṃbramī* 190.10, and (with *i*) *darśayañi* 49.7, *śārayi* 51.8, *śruṇi* 90.4, *kṣipī* 94.12, *abhyayañi* 191.8, *abhāñi* 194.3, *pravarañi* 331.1, *drōpayi* 331.11 (misprinted); in LV, *kṣipī* 92.16, 338.21, *vināpti* 93.2, *anadārami* 94.17, *nīcāñi* 175.16, 182.21, 183.3, 4, 6, 8, 10, 11, *pati* 194.10, *lekhi* 197.1 (so with v.l., for ed. *lekhi*, to \**lekhiñi*, see note 4), *vādhayī* 222.6, *nyasi* 236.14, *pravarañi* 285.6, *abhiñi* 329.4, *śnamī* (*ava-nam*) 356.19, *prcchī* 421.13; and (with *i*) *upagami* 49.7, 50.11, *vidarśi* 49.10, *patyī* 75.15, *vīkṣi* and *vīkṣiñi* 75.16, *viśarjayañi* *nīvēśayañi* 158.13, *chādayi* 158.19, *anurasi* (so, for *latu rui*) 164.22, *manāñi* 236.22, *avasi* 241.1, 339.21, *abhiñi* 342.3 (*jānati* = *jānati*), *abhāñi* 349.4, *adhyañi* 416.3.

§ 17. *Distinctive 3 plural forms.* The *i(i)* is often used with 3 pl. subjects, we find also quite commonly Prakritic 3 pl. forms related to the Skt. ending *īṣuḥ*. They are formed, like the *i(i)* forms, from present bases in thematic *a* which is dropped. In AMg. the ending appears in the form *īṣu*; in Pali both in this form and as *īṣuṃ*. In the Prakrit which underlies our dialect the commonest ending was probably *īṣu*, the direct phonetic heir of Skt. *īṣuḥ*. Here it appears normally as *īṣu*, rarely as *īṣu* (our mss. very often write *s* for *ṣ* in other forms, so that the variation in sibilants is probably of no specific significance). It is thus different from Pali in never showing final anusvāra. On the contrary, when the meter requires a long final syllable, neither the Pali form nor the Skt. is used, but (regularly) the *u* is lengthened to *ā*. When the meter requires a long penult, the vowel *i* may be lengthened to *ī*. But more often the alternative ending is used which corresponds to the exclusive AMg. and alternative Pali ending *īṣuṃ*. This appears in our dialect, at least in the mss., in a remarkable variety of spellings. Aside from the normal lengthening of final *u* (when metrically required), we find also, tho rarely, the Skt. ending *uḥ* in its various sandhi forms. The sibilant before it is almost always written *s*, rarely *ṣ*, contrasting remarkably with the alternative ending which

is normally written *isu*, rarely *isū*. Instead of the anusvāra before the sibilant we find the consonant *s* quite frequently, and sometimes—if we may trust the editors' statements—even *ṣ*. And finally, for the penultimate vowel *i* we sometimes find *e* written. Thus, altogether, we find the following array of endings: *isu*, *isū*, *isu*, *isū*, *isē* (all to be regarded as variants of one form of the ending, corresponding to Pali *issuṃ*); and *imsu*, *ipsū*, *isṣū*, *insu*, *insuḥ* ("suḥ ca"), *itsu*, *ensu*, *amsu* (all variants of the alternative ending corresponding to Pali and AMg. *issu*). We shall treat these two groups separately.

§ 18. 1. *isu* and congeners. The fundamental form is clearly *isu*; the others are variants, chiefly m. c. (on the writing *s* for *ṣ* see above). Almost limited to verses. In the prose of LV has been noted only (397. 21-2) *antaradhāyisu* (so probably to be read with the best ms.) or *ṣuḥ* (so Lefmann with other mss.), "disappeared". This would in Skt. be a passive, *antar-adhāyanta*; our form is to be connected with the Pali present *antara-dhāyati*, and what we have is probably a Prakritic *antarc-dhāyisu* (so, rather than *antar-adhāyisu*). In the prose text of SP as printed I have noted no case, but the Kashgar mss. sometimes show such forms (examples above, § 5). The rest are all in verses. The vowels *i* and *u* are always short in metrically indifferent positions.

*isu*: *paṣyisu* SP 194. 7 (no v. l.), *snapayisu* LV 234. 4 (the only v. l. is *ṣipṣasu* which is unmetrical).

*isu*: in SP, *kurṣisu* 10. 6, *paśyisu* 15. 5, 325. 6, *krīḍisu* 50. 12, *krandisu* 84. 12. In LV, *hīṃsasu* 28. 6, *gacchisu* 74. 6, *drāyisu* 74. 12, *utḍisu* 75. 13, *rohisu* 75. 20, *nandisu* 76. 2, *namisu* 92. 8, 158. 7, *udāṣisu* 114. 18, *abhiwandisu* 116. 4, *pradhāyisu* 164. 20, *ravisu* 167. 12 (so, or *ravisu*, with mss.), *pratibodhayisu* 173. 9, *paricartisu* 173. 16, *muñcisū* 193. 17, *adāyisu* 194. 2, *varṣisu* 222. 7, *prekṣisu* 240. 18, *prapaṭisu* 329. 13, *stāvisu* 339. 14, *nirīkṣisu* 340. 8, *bhāṣisu* 352. 20, *pūrīsu* 357. 3, *udāharisu* 413. 21, *adhyeṣisu* 414. 10 (so for ad. *ṣu*).

*isū*: in SP, *parikhāyisū* (-hāyate) 170. 2, *adhyeṣisū* 190. 11, 191. 3, *paṣyisū* 323. 14. In LV, *gacchisū* 74. 10, *vivocisū* 122. 22, *vrajisū* 169. 12, *muñcisū* (must be read m. c. for *ṣu*) 217. 1, *buddhisū* (see § 14, end) 230. 12, *ranisū* 222. 7, *abhiwandisū* 236. 12, *bādhisū* (must be read m. c. for *ṣu*) 357. 6.

*tsu*: varfissu LV 408.14. (In *udgṛhṣiṣu*, v.l. of O in SP 181.9, prose, the *ṣ* is to be explained—if the form be accepted—as a remembrance of the Skt. present stem *°gṛhṣīṣ*.)  
*tsā*: *kāṣṭhapaṣṭū* SP 50.16 (no v.l.) and 51.1 (v.l. of O *°yissu*).  
*issu*: the normal Skt. ending, in a form otherwise Prakritic, when the meter requires a long ultima, has been noticed only once: *kṣipissu* LV 282.16. Based on the present *kṣipati*, like the other forms of this section.

§ 19. 2. Much rarer are the forms of the alternative ending, with nasalized penultimate vowel, corresponding to AMg. and (alternatively) Pali *issu*. But the variations in spelling are perhaps even more numerous and curious. The lengthening of final *u* m. c. calls for no comment. The sibilant is, this time, almost invariably written *ṣ*, a clear indication of direct inheritance from a Prakrit dialect. The preceding nasal seems to be written *n* almost as often as *m*; but this may be merely a matter of orthography. At least similar variations occur in other forms, outside of this ending; there is a great deal of confusion in the mss. between *n* (and *m*) and *m*, both internal and final. Further, even *t* is written for *n* (or *m*). On this see Senart Mhv. I. xvi, who thinks of the Vedic insertion of *t* between (final) *n* and (initial) *s*, but also suggests that *t* for *n* in our texts may be largely or wholly a matter of graphic confusion, due to the similarity of the signs for *n* and *t* before consonants. If this last is the true explanation, there would be no real linguistic importance in the matter. Our texts, apparently, present *t* for *n* very rarely. But the mss. of the Mahāvastu, contrariwise, are stated by Senart to read *t* very commonly, even regularly, for *n* in these 3 pl. forms; nevertheless Senart himself in his edition prints uniformly *n*. Puzzling also is the writing (extremely rare in our texts but very common in Mhv., see Senart l. c.) of *e* for the penultimate *i*. Senart refers to "Pali and Prakrit forms in *essu* beside *issu*". I find no record of such forms in Pali; and Pischel § 516 considers the true AMg. ending *issu*, tho he records the occasional spelling *essu*, regarding it merely as a Prakritic writing for *issu* (cf. his § 119). Aside from *sādhessu* LV 337.1 (which in the first place is an emendation, tho I think a plausible one, and in the second place belongs to *sādhessati*, Pali *sādhessati*, and may have its *e* by contamination from such present forms), I have found in SP and LV only the isolated *śruṇessu* SP 53.12 (with no v.l.); cf. Mhv. I. 42.

12 *śṛṅgeṣuḥ* (mas. all °*ṣuḥ*), and many similar forms in Mhv. Three explanations of the *e* for *i* seem to me conceivable: (1) that given by Pischel, above, for AMg. *ṣṛṣu* = *iṣu*; (2) influence of stems in Prakritic *e* from *aya*, cf. *sādhaṃsu* just quoted, and cf. further the great extension of such stems in *e* in Prakrit generally (Pischel § 472); (3) influence of the 3 sg. forms in *e* (originally optative?) which seem undoubtedly to be used as *uorists*, see § 32 below. Of these explanations the first or the second will be most apt to be right.

§ 20. No forms of this type occur in the prose of the editions as printed. But for *parāhananti* SP 69. 11 (prose), O reads °*hanitsu* (for °*haninsu*, see above); for *abhāṣātām* SP 267. 2 (prose), LaVP 1074 has *bhāṣinsuḥ*; and several such readings occur in the Hoerle fragments (Lüders 152 *upasaṃkraminsu* for SP 270. 11 and *nāḍinsu* for SP 271. 3; Thomas 134 *pravartayinsu* for SP 327. 11, and 136 *prādurbhaviṃsu* for SP 329. 2, *prapatinsu* for SP 329. 3). I have little doubt that these semi-Prakritic forms are all closer to the original readings than the correct Skt. forms of the Nepalese mss. adopted in the editions.—The final *h* (or equivalent) of Skt. is preserved (as v. l., that is in some mss.) in the prose *bhāṣinsuḥ* of LaVP above, and in *niryādayinsu* ca; v. l. of O in SP 191. 2 (verse).—The remaining occurrences are all in verses.

*iṣuḥ*: *raṇiṣuḥ* LV 233. 18. All mss. have *ṣ* (Calc. *raṇiṣu*, which is unmetrical). This is the only case I have noted containing *s* rather than *ṣ*; note that in the same line occurs *patiṃsu* with the usual *s*.

*iṣu*: *abhiṣṭayinsu* SP 191. 3; *avacinsu* SP 192. 10 (O °*asu*, one Nepalese ms. °*ṭsu*); *śṛṇvayinsu* SP 194. 5; *nyasyinsu*, probably to be read at LV 222. 8 (from *ny-asyati*, "cast down"; the mss. vary greatly; Lefmann *nyasayinsu*, which would also be possible metrically); *abhaṣayinsu* LV 353. 6; *stavyinsu* LV 414. 10 (Calc. °*yitsu*).

*iṣu*: *saṃśṛṇvayinsu* (ed. °*su*, unmetrical; O °*asu*) SP 194. 2; *kṛpīṃsu* . . . *bhramayinsu* LV 81. 7; *patiṃsu* LV 233. 18; *staviṃsu* LV 233. 19.

*insu* (for prose cases see above): in SP, *darsinsu* 55. 13 (some Nepalese mss. °*ṣinsu*, others °*ḥinsu*); in 50. 3 ff. read probably *karinsu* (several times) with O; O also has v. l. *kārāpayinsu* 51. 1, *śikṣinsu* 51. 6. In LV, *śaṇṇinsu* 93. 21 and 23 (for the



latter all mss. except the best °*itsu*), *apagamsinu* 94. 21 (v. l. °*itsu*).

*iesā?*: in SP 51. 8 O has v. l. *saṃdāpīnu*, which is unmetrical (°*sā* would be required), for ed. *saṃdāpenta*.

*amāu* and *amū*, see above, § 19, end.

§ 21. Other (non-Sanskritic) *i*-aorist forms. There remain to be mentioned a few stray forms, most of which are not particularly abnormal in formation but are not recorded in Sanskrit. Thus, *anamiṣṭa* LV 295. 7 would be (but for the Prakritic phonology of the preverb) a regular 3 sg. middle *i*-aorist from *am-nam*; but the root *nam* is not authorized to form an *i*-aorist in Sanskrit. And *vilambīṣṭhāḥ* LV 217. 11 (prose) would likewise be a regular 2 sg. middle; in Skt. *ālambīṣṭa* is quoted from grammarians but according to Whitney's *Roots* not recorded in literature. While *staviṣṭa* LV 238. 6 is regular Sanskrit in that form (but for lack of augment and the fact that the subject is 3 pl.); but (the no v. l. is recorded) the meter requires *stāvīṣṭ* (the final *a* must be elided before the following initial *a*-), with *vṛddhi* root-vowel, a transfer from the vocalism of the active (or lengthening m. c.<sup>2</sup>). More anomalous is *nyasīt* LV 271. 19, apparently "deposited, provided" (*bhadrā-sanaṃ*; Foucaux *apprête*), from *nā-as* "throw." This root has no *i*-aor. active in Skt.; if one existed it would be \**āsīt* (homonymous with *āsīt* "was"); the grammarians quote a middle *āśīṣṭa*. The *ṣ* for *s* is anomalous, but apparently found in all Lefmann's mss.; only Calc. has *nyasīt*. I have noticed no other case of *ṣ* for regular *s*, the *s* for *ṣ* is frequent. Even *ṣ* for *ś* is rare. Accordingly I am inclined to regard *ṣ* here not as a phonetic confusion of sibilants but as an analogic change, due to the influence of *s*-aorist forms in *ṣīt* such as *āśraṇṣīt*, *anāṣīt*, in which the sibilant belongs to the aorist suffix, not to the root as here, and follows a non-*a* vowel. The short *e* is of course also anomalous but may be regarded as m. c.

§ 22. The inherited Skt. imperfect *āsīs*, *āsīt*, "was", is treated in our language like an *i*-aorist, as we might expect (in fact it was originally formed in imitation of this type). It may appear as *āsi*, as in LV 168. 15, 169. 1, 9 (in all three most mss. *āsīd* [or with *ś* for *s*], tho with 2 sg. subject), or, when the meter requires a short, as *āsi* (SP 27. 6, LV 76. 1, 194. 3; with 2 sg. subject SP 312. 15 [? here perhaps *asi*, pres., is intended], LV 167. 21; with 1 sg. subject SP 63. 13).

II. Descendants of Sanskrit *s*-aorists, and similar forms

§ 23. Geiger § 167 (cf. 163, 165. 2) noticed that Pali, which freely forms aorists of the *ī*-type from present stems in general (he should have said, I think, from thematic presents), does not form them from stems in final long vowel, but in such cases has forms which look like Skt. *s*-aorists. Our language seems to follow the same principle in general; and Pischel § 516 contains material enough to suggest that it prevailed in AMg. too, tho he does not formulate it. It would hardly be going too far to say that in all three, the general rule is to form *s*-aorists from bases (not only present stems, in fact more often "roots" in our dialect) ending in vowels (or at least in long vowels), while otherwise, and particularly from thematic presents, *ī*-aorists are made (after dropping the thematic vowel).

§ 24. We saw above that when *apa* of causative and denominative stems is contracted to *e*, *s*-aorists are made from such stems. So *thapeṣi* LV 169. 3 (with dental *s*, perhaps—like loss of initial *e*—pointing directly to a Prakrit form, but *s* for both *s* and *ś* is very frequent), from *sthapayati*; and *kīrteṣy* SP 63. 9 (with cerebral *s*; read with WT *kīrteṣy atitān*). From the Prakritic standpoint, these forms fall into a common category with *parigraheṣi* SP 204. 8, cf. Pali *aggāheṣi*, altho historically the *e* here is doubtless an inheritance from Skt. *aī*, cf. *agrakāśam* AB. etc. Pali has aorists in *esi* of still different historic origins (Geiger § 165). And AMg. has, besides forms from *apa* verbs like *kakeṣi* (: *kathayati*), also *anneṣi* (\**aññāyīṣi*) from *jān*, *aheṣi* (\**abhāṣi*) from *bhān* (note that the BSkt. aorist of *bhā* is different, see below), which historically stem from *ī*-aorists but by Prakrit phonetic changes have fallen into this group; and perhaps others.

§ 25. Another *s*-aorist form, after a vowel, is *asraṣi*, twice in the prose of LV (143. 13, 300. 11-12), a slightly Prakritized equivalent of Skt. *āśraṣi*. Note further *samavāṭṭiṣṭhaṣi* v. l. of O (probably to be adopted) for ed. *samavāṭṭiṣṭhaṣi* SP 230. 3 (prose); *adūṣi* SP 250. 3 (prose; no v. l.); and in a verse *asthāṣi* SP 66. 4. And finally, the very common *abhāṣi*, which points to a protocanonical Prakrit aorist of *bhā* different from any found in either Pali or AMg., which (aside from non-sibilant aorists) show only *akeṣi* (also *aheṣwā*) and *aheṣi* respectively. If the protocanonical Pkt. had

agreed with them, we should find \**abhaṣīt* or \**abhaṣī* (or °*ṣī*, °*ṣī*), which have not been found. This is the more surprising because we should expect parallelism with the future, which in BSkt. actually does appear as *bhaṣyati* (regularly) or *bhaṣyati* (rarely). Another surprising thing about this form is that, while in prose the form *abhaṣīt* occurs (at least in the Kashgar recension of SP), *abhaṣī* seems not clearly to be found except in verses and there only in positions where meter requires a long final (see note 3 above), and the regular form seems to have been *abhaṣī* with short *i*. This occurs in metrically indifferent positions SP 27. 4, 93. 3, 193. 5; and according to the Lüders fragment, Hoernle 150 *infra*, once even in prose (this word is omitted altogether in the edition of SP). Other occurrences in verses: SP 26. 13, 56. 11, 12, 67. 5, 157. 11, 204. 11, 354. 2; with 3 pl. subjects, 49. 14, 50. 1, 2, 6, 12, 14, 16, 51. 2, 4, 6, 14, 52. 3, 13, 190. 15. In LV it is much rarer: 27. 19, 123. 8 (read *abhaṣī* with best ms. A for *subhā*°), 163. 16, 415. 6.

§ 26. After consonants, *s*-aorists are rare except when they preserve normal Skt. forms. In SP 468. 7 (prose), a late passage, the edition reads *akṣaipṣīt* (from *kṣip*), a form quoted by Hindu grammarians but not known in literature (Whitney, *Roots*); but the reading is very uncertain; only three Nepalese mss. have the word at all, and of these only one reads *akṣaipṣīt* (the others *akṣe*°, *akṣt*°), while O (the Kashgar recension) has *kṣipataḥ*, a form which I do not understand. Note further *pravaṣīt* LV 196. 6 (in a verse); *viṣ* has no *s*-aor. in Skt.; if it existed it should be \**maṣīṣīt*.—LV 364. 11 has a 3 pl. aorist *dikṣiṣu*, from *diṣ*. Formally it could be described as a “*siṣ*” aorist (Whitney’s 6th class). But its nearest relative among recorded forms is the (7th or *sa*-) aorist *adikṣat*, of which the 3 pl. *adikṣan* has here been furnished with the Prakritic 3 pl. ending customary in our dialect.—Yet more anomalous is *lapṣi* SP 190. 2, aorist to *labh*. Whitney, *Roots*, mentions “*alabāḥa*, *alapsata* B.; *lapsiṇa* S.”; *lapṣi* might pass for a modification of this type by giving it the normal Prakritic ending. One is almost tempted to assume contamination from the desiderative (*lipṣate*, *lipṣati*).

§ 27. Apparent exceptions, showing *s*-aorists after a consonant, are further such forms as *vyākharṣu* “they predicted” SP 27. 3; *vyākharṣīt*, probably the true reading underlying the corrupt *vyākharṣitaḥ* in LV 39. 19 (prose), and *harṣur* “they removed” LV 271. 16.

In both Pali (Geiger § 163.4) and AMg. (Pischel § 516) such roots (in original *r*) show aorists *akāsi*, *akāsi*, which conform to our rule, since they resemble roots in *ā*, tho of course historically derived from Skt. *akāryā*, *akāryā*. It might be suggested that our forms are direct inheritances from these Skt. forms, with *ā* shortened to *a* by Prakrit phonology before a consonant cluster. But, rather surprisingly, I have not found evidence for such shortening in our dialect, at least in the orthography of our mss. It is more likely that *vyākāryā* is an imperfect Sanskritization of *\*kāsī* of protocanonical Pkt., influenced by other forms in *kar*-. Cf. such aorist forms as *vyākari* LV 393.12, *kari* LV 200.9, *udākariṣu* LV 413.21, based on the thematic presents *karati* and *karati* (the former is found e. g. in *kareya* LV 335.19, *sathareta* SP 223.14).

§ 28. The blend-form (*a*)*drāsī*, etc. We find also (formally in accord with our rule) certain complicated and interesting blend-forms from the root *drś* which agree closely with Pali forms, and (presumably like them) must stem from the protocanonical Prakrit. They are ultimately based on the old thematic aorist *adrāsāt*; see Geiger §§ 162.3, 165.1, and our note 4 above. When augmented, the forms, tho usually written with single *ā*, are always pronounced *add-* as the meter proves. On the basis of Pali *addasā* is formed Pali *addasāsi*, 3 sg., with *s*-aorist termination; Geiger § 165.1 correctly explains the form as based on the analogy of *akā* : *akāsi*, *adā* : *adāsi*, all aorists (from *ky* and *ad*). It appears in our dialect, half-Sanskritized, as (3 sg.) *adrāsī* (pronounced *add<sup>a</sup>*) LV 196.12, 15, 19. In the same context the same form occurs as 2 sg., LV 195.11, 15, 19. Historically it can be justified as both 2 and 3 sg. (Skt. *\*sī* as well as *\*sī*). Since however the form in *-si* looks like a 3 sg. present, we also find an analogical 3 sg. in *-ti* : *adrāsīti* (pron. *add<sup>a</sup>*) LV 75.6, 194.20, 197.9, 241.10. Very similarly Pali shows a 1 sg. *addasāmi* (Geiger § 163.3). It is likely that in Pali, as in our dialect, these are direct inheritances from the protocanonical Prakrit, that is not native to the Pali dialect. Perhaps the blend forms implying interpretations as presents, with primary endings, may have been assisted by the fact that there was no present from this root in Sanskrit or Pali, or presumably in the protocanonical Prakrit (forms of *paś* being substituted); and since the augment was often if not usually lost, the association of such forms with past time must have been weak. In other words,

the bland forms with "primary" endings may have been felt as presents, not preterites.

§ 29. Finally, in SP 51.6 occurs a strange form *kr̥tāsi* (it may also be understood as *akr̥tāsi*), which, if the true reading, can hardly be anything but a 3 sg. aorist (with 3 pl. subject) to *kr̥*. The edition with all Nepalese mss. reads *sakhena kāsikena kr̥tāsi vīgrahāna*, "(who) with fingernail or stick have made images (of the Buddha)." The Kashgar mss. have a quite different reading, containing the verb-form *ālikhāsu*, from *ā-likh*; this may possibly be the true reading. WT emend to *kr̥tāvi* (= *\*r̥ivah*, nom. pl. to *kr̥tāvin*, as periphrastic perfect); but on paleographic grounds the emendation is not attractive. Perhaps (a)*kr̥tāsi* may be a blend-form like *adr̥śāsi*, based on *akr̥ta*, 3 sg. aor. middle. Pati makes frequent use of such 3 sg. middles, ending in *tha* (with aspirate instead of Skt. *ta*, cf. Geiger § 129); e.g. *ajāyatha* etc., Geiger § 161. In view of these forms and Pali *abā* (3 sg. aor., cf. Vedic *akar*), it is not hard to suppose a Pkt. *\*akata* (or *\*akatha*), from which *\*akatāsi* would be produced just like Pali *addassasi* to *addasa*; this would then be half-Sanskritized in our dialect as (a)*kr̥tāsi*.

### III. Prakritic descendants of the root aorist

§ 30. Here we need note only forms of the root *bhū*. The 3 sg. *abhat* may be used as such for other persons and numbers, as regularly with all 3 sg. forms in our dialect. So even in prose, (a)*bhāt* as 1 sg. SP 22.11, LV 239.18, and as 2 sg. SP 64.11. And in verses, as 3 pl., SP 46.5, LV 74.16, 138.3, 164.1. By Prakritic phonology the *t* may be dropped, leaving *abhū*; and that even before a following initial vowel, leaving hiatus, which would have been avoided by keeping the Sanskrit form, SP 157.10. The same (a)*bhū* is used as 2 sg. (here perhaps directly representing Skt. *abhūḥ*) LV 165.13, 17.21; 166.3, 7, 11, 15; 170.19; and as 1 pl. LV 262.11 ("we [gods] were invited by you of old to the Sacrifice of the Good Law"; misunderstood by Foucaux). Furthermore, the final *ū* may be shortened m. e., yielding *abha*: SP 25.10, 204.9, LV 237.14.

§ 31. Once in the prose of LV (83.13) occurs a 3 dual *abhāvaṃ*, for *abhatvām*, with insertion of thematic *a*. This may be taken as a kind of blend between the regular root-aorist and the

thematic aorist (according to Geiger, imperfect) *abhuwat* (in Skt. only Vedic) = Pali *abhuwā* (Geiger § 162. 2). Weller (*Ueber die Prosa des Lalita Vistara* 52) proposes to emend the text to *abhu-wāḥen*, bringing it into direct agreement with such Pali forms. But this is hardly necessary. Weller exaggerates when he quotes Senart (Mhv. I. xii) as saying that there is "no difference" between the writing of *ā* and *u* in Nepalese mss.; what Senart says is rather that it is "very often impossible to distinguish with certainty" between *ā* and *u*. It is however true that LV 222. 11 and 21 (both verses) show a form *bhuw'* (*imw*; before initial *i*), a 3 ag. preterite which might be directly equated with Pali (a)*bhuwā*, Vedic *abhuwat*. Cf. also AMg. *bhuvi*(*u*), Pischel § 516; *bhuw'* in our dialect could stand equally well for (a)*bhuwā*(*t*) or (a)*bhuwi* (= \**abhuwūt*; note the following *i-l*).

#### IV. Prakritic descendants of imperfect and a-aorist

§ 32. In Pali the imperfect has fused with the aorist, particularly the thematic aorist (Geiger § 169, II, IV), and all preterite 3 pl. forms end in *uṇ*, which of course corresponds to Skt. *uṭ*. In our language we find a few 3 pl. preterite forms in *uṭ*, from thematic stems, which deserve mention in this connection. In footnote 4 above, mention was made of *adṛu* (m. c. for \**śuṭ*; written with *s* for *ś* as often) LV 27. 22, corresponding to Pali *addasaṇi*, Skt. *adṛśan*. Another clear case is *vimṛuṭ* LV 385. 19, for *vimṛśuṭ*, 3 pl. preterite to *vimṛśati* (Skt. *vy-amṛśan*). In the light of these forms I have little hesitation in interpreting thus (rather than as optative 3 pl., for *deśayeyuṭ*) the form *deśayyuṭ*, which occurs once or twice in textually rather uncertain passages. In SP 57. 16 the edition reads so, probably rightly; O °*giśhu*, intending °*giśu*, which is impossible metrically but confirms the interpretation as preterite 3 pl. In SP 272. 10 the ed. also has *deśayyuṭ* without report of v. l.; see § 36 for the Kashgar reading *deśayt*, which suggests that if *deśayyuṭ* was the true reading it belonged formally to the aorists, even if optative (or future?) meaning must be attributed to it.

#### V. Blends of aorist and optative forms (also with future meaning)

§ 33. Pischel § 466, end, presents convincing evidence to show that in AMg., "so unerklärlich es scheint," forms which can hardly be other than optatives are used in the sense of past indicatives.



Among these he mentions 3 sg. forms in *e* (Skt. *et*). One such form has turned up in our texts: *spṛśe sa bodhitaḥ* SP 190.7, "he attained enlightenment." The meaning is certain; it occurs in the description of how a past Buddha attained his goal. Some equally clear cases occur in the Mahāvastu, e. g. iii. 53. 18 (prose) *prajñāpayed aham . . . nīside bhagavān*, "I offered . . . the Lord sat down." Here *nīside* (better read °*det*, see critical note) is 3 sg., and *prajñāpayed* 3 sg. used as 1 sg. (unless the -*d* be considered a hiatus-bridger); simple narrative preterites are meant, despite the apparently optative forms.

§ 34. Not recorded in Prakrit or Pali, so far as I can see, is the reverse phenomenon, which is commoner in SP and LV, namely the use of forms in *i* (also *ī*, m. c., which however can usually be taken as a normal optative with *i* for *e*) which look like 3 sg. aorists, but have the meaning of optatives (either jussive or potential), or even apparently of simple futures. Since the optative approaches very closely the meaning of the future at times, all these may perhaps be classed as "optative" in meaning. We may recall that both *e* (opt.) and *i* (aor.), from Skt. *et* and *it*, are often reduced to short *i* in our dialect, almost always to be sure when the meter requires a short. Does this fact lie at the root of this confusion and that mentioned in the last paragraph? At all events it seems impossible to deny the facts as stated.

§ 35. Thus the following forms in *i* (once *it*) cannot have past meaning, and appear to be optatives in force:

*nābhāsi* LV 122. 16 "would not shine" (a parallel *bhavet* occurs in the same stanza).

*nābhāsi na tapī* LV 122. 19 (also closely attended by parallel *bhavet*).

*praviṣṣi bhavana(h)* LV 115. 4 "let your worships enter."

*sarveṣu maitribalaḥ so hi darśayī* SP 291. 12 "he shall show power of kindness to all (beings)." An injunction.

*vyākartī* LV 57. 3 "(brahmins who) shall (or might, may, could?) explain (my dream)"; Māyā speaks to the king.

*niścari* LV 177. 9, and *ārī* 177. 12, "would (ever) depart . . . see;" apparently optatives of generalized statement.

*na śraddadhi mahyam imān vibhāṣitān pītā mamāyaṇā hi na cāpi śraddadhi* SP 113. 10, "he would (will?) not believe in

this magnificence (so Kern; Tibetan *ābyar-ḥa* [read -paʔ] 'wealth') of mine, nor yet would he believe, 'this man is my father.' " In the parable of the Prodigal Son. Since the father has not yet declared himself and the son has no reason yet to think of him as such, surely a past cannot be meant.

§ 36. The last two examples approach the borderline of the future. In the following the most natural interpretation would be as simple futures:

*uddhavi* LV 195. 12, 196. 2, and *sampraśyī* 195. 30, in a passage containing predictions. Parallel to the latter (*mām sampraśyī* "will see me") is *mām draṣṭāsi* 195. 18 (periphrastic future, with *i* for *ī* m. c.), and all the parallel verbs are either futures or presents with future meaning. Cf. also *vyaktam kṛmāva abhiniṣkramī* (short *i* m. c.) *bodhīketoḥ* LV 135. 14, "the prince is clearly going to go forth in search of enlightenment" (a definite prediction; if *\*kramī* stands for the optative *\*krame[t]*, the opt. has certainly merged pretty completely into the future in meaning).

The verses of SP xii (2-21), SP 271 ff., contain promises and predictions. Definitely future verbs are the rule, with occasional present forms in future meaning. But precisely parallel with them are *deśayī* 272. 5, and according to LaVP 1976 also 372. 10 (where the edition has *deśayak*); *cārayī* 272. 3; according to LaVP l. c. *prakāśayāt* in 272. 10 for edition *\*ya*.

Again in another passage SP p. 295 we find *samprakāśayī* line 2, *abhinīṣkramī* and *upasaṁkramī* 4, *pravartayī* 7, *deśayī* 8. Here most of the parallel verbs are present in form, and might possibly be interpreted as generalized statements (a meaning which would go with the optative). But *anupratīpsyate*, line 6, is future, and suggests that the whole passage is rather concerned with definite prophecy, which is really more appropriate. If these forms are "optatives", the optative meaning is stretched a good deal. But still less can they be considered preterites; yet formally they look like aorists.

# THE ROOT FORMS $si(m)$ AND $su_{11}(m)$ , "TO GIVE," IN SUMERIAN

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IN A FOOTNOTE added to his review<sup>1</sup> of my publication *The Sumerian Prefix Forms e- and i- in the Time of the Earlier Princes of Lagash*,<sup>2</sup> Thureau-Dangin remarks that the reading of the Sumerian verb form  $i-si$ , "he has given," as  $i-si(m)$  (of the root  $sim$ , "to give") "n'est fondée que sur  $S^b$  dont le témoignage a été de tout temps sujet à caution en raison du fréquent complément - $mu$ . La véritable lecture ( $sum$ ) est maintenant donnée par le Vocabulaire de Chicago. Il faut lire  $i-sum$  et distinguer nettement  $si$ ,  $si(g) = nadû$  de  $sum$ ,  $sûn$  (dialect  $zem$ ) —  $nadûnu$ ."<sup>3</sup> Thureau-Dangin thus makes the following assertions:

(1).  $S^b$ , No. 2 (= CT 11, 24 ff.), col. 3, l. 7, is wrong in attributing the reading  $si-i$  to  $si = nadûnu$ .

(2). The real pronunciation of  $si = nadûnu$  is given by Chicago Syllabary, l. 123, namely, as  $sum$ , with a variant pronunciation  $sum$  as proved by the sign name  $sumu$ .

(3). The reading of  $si = nadûnu$  as  $sum$  instead of  $si$  is proved

<sup>1</sup> Published in AJSL 49 (1932-33), 269 f. The article here published was written four years ago (1932) immediately upon receiving Thureau-Dangin's review.

<sup>2</sup> Published as No. 3 of the Chicago Oriental Institute series "Assyriological Studies."

<sup>3</sup> Thureau-Dangin begins the note with the words: "Poebel donne p. 2 un tableau des formes de préfixe e ou i, en ne retenant que celles 'in which the vowel of the root is undoubtedly an a, e, i, or u, leaving aside for the moment all those cases in which either the root itself or the vowel is in doubt.' Or, parmi ces formes figure  $i-si(m)$  où la voyelle est en premier chef incertaine." I wish to point out that, as the context clearly shows, the "uncertainty" debarred by the condition mentioned above refers only to those verbal roots whose vowel is not definitely either the open a, which causes the prefix to appear as e-, or one of the close vowels i, u, e, which cause the prefix to appear as i-. The form  $i-si(m)$ , however, whether its root is to be pronounced  $si(m)$  (which I think was the preferred pronunciation at the time of the early princes of Lagash) or  $sum$  (which Thureau-Dangin recognizes as the only correct pronunciation), is rightly listed in the second group; for in either case the root contains a close vowel.

by the fact that *si* is frequently followed by the so-called "phonetic complement" -*mu*.

(4). The *m* of the root form *sum*, as well as the *w* of the root form *sun*, "to give," is inamissible.<sup>4</sup>

(5). The reading *si* (written *si-i* in the glossing of the syllabary) represents exclusively the first part of the root or roots *sig*, which Thureau-Dangin illustrates by the verb *si*, *si(g)* (i. e., the verb *sig* with dropped or preserved final *g*) = *nadû*, "to throw."

From Thureau-Dangin's remark it might seem that my own opinion is that the pronunciation of the root *si* = *nadûnu* was exclusively *si(m)*. A glance at the alphabetic list of verbal forms at the end of the study on the prefix forms, however, will show that this is not the case, for on page 49 of that study the Sumerian verb for "to give" is registered not only in the form *si(m)*, but also in the form *su<sub>11</sub>(m)*, the latter form being based, of course, on Chicago Syllabary 127, that is, the very passage referred to by Thureau-Dangin in his review. Note, moreover, my remarks in GSG (1923), § 18, where I left it an open question as to which of the roots *si(m)* and *sum*—both attested by syllabaries—is the more original.

As a matter of fact, my position is this, that both pronunciations existed, although it is my belief that *si(m)* represents a more original form of the root and, moreover, was the form preferred, or even regularly used, in the Old Sumerian period. It is the purpose of the following discussion to prove the correctness of this view.

#### 1. The Attestation of *si(m)* = *nadûnu*

In order to make ourselves acquainted first with the general aspect of the problem, the fact must once more be stressed that the pronunciations of the root *si(m)*, "to give," as *si(m)* and *su<sub>11</sub>(m)* are both attested by Babylonian vocabularies, the former if for the present we confine ourselves to the evidence quoted by Thureau-Dangin, by the equation in S<sup>b</sup>, No. 3 (= CT XI 24 f.), col. 3, l. 7,

| *si-i*                      | *si*                      | *na-du-an*

<sup>4</sup> That this is Thureau-Dangin's assumption follows from the fact that he quotes the root exclusively as *sum* and *sun*, not as *su<sub>11</sub>*, *su<sub>11</sub>(m)* or *su<sub>11</sub>(m)*. Note, on the other hand, that he gives the root of the Sumerian equivalent for *nadû* as *si* and *si(g)*, i. e., the root *sig* with dropped final *g*.

and the latter by Chicago Syllabary, l. 123

† su-am      † si      † " (= su-su)      † su-da-su

In his review Thureau-Dangin seems to set great value on the presumed fact that the pronunciation *si(m)* of the root *si(m)*, "to give," is founded only on the quoted passage in S<sup>3</sup>, No. 2. However, it can readily be shown that it is found also in S<sup>c</sup>: DT 40 (= CT 11, 29 ff.). The partly broken passage, col. 1, l. 30—col. 2, l. 3, of this syllabary, which treats of the sign *si*, may be restored as follows:

20 [†a]i-i	si	su-su-su	ha-a-d[i] su-su-su-m[u] ta-ma-hu su-[.....] su-[.....] su-[da-su] su-da-[u ša .....] su-[.....] su-g[da-su] ta-ma-hu ša [.....] ha-su...[...] su-su-su ša [.....] su-pa-[su]
11 [†si]-i	[si]	su-su-su	

It will be noted that the first two Akkadian equivalents of *si* — *si* (written *si-i*) in this passage, namely *hādu*, "to give," "to bestow," and *šutlumū*, "to give (something to someone)," "to bestow (something on someone)," are direct synonyms of *nadānu*, "to give"; and unless it can be proved absolutely by other evidence that the Sumerian equivalents of the two verbs were different from *si(m)* = *nadānu*, we have, of course, to assume that our *si-i* in the meaning of *hādu* and *šutlumū* is intended to represent the root *si(m)*, "to give."

The next equivalent *tamāhu* ordinarily means "to grasp," a meaning, however, which cannot be brought into accord with any Sumerian verb root beginning with *si*. But if we assume that this line is taken from a syllabary whose scribe was accustomed to quote the verbs in the I 1 formation even in cases where an exact correspondence would be achieved only by a III 1 form, it would be quite regular. For *šutlumū*, "to cause (someone) to take (something)," is again a direct synonym of *nadānu*, "to give." Note especially that it was evidently this parallelism of meaning between *šutlumū*

and *nadānu* that has even led to a construction of the former verb which parallels that of *nadānu*, namely, *x ana gaššū y šuṣṣu*, "to give (or place) *x* in *y*'s hand." Indeed, a direct equation of Sumerian *si(m)* and Akkadian *lamāḫa* would result if, with Delitzsch, *Handwörterbuch*, p. 709, a meaning "to give" could be actually assumed for *lamāḫa* itself. But the sense of the passage Aššur-naṣir-apli, 1 R 45 ff., col. 1,<sup>10</sup> evidently is that Aššur, himself taking or grasping his weapon, stood as helper at the side of the king. At any rate there can be no doubt that in the third line of our passage too *si* (— *si-i*) represents the shortened root *si(m)*, "to give."

For the seventh equivalent *na-dū*[.....] no other restoration in its first part is possible than the verb *nadû*, which in the texts not infrequently appears as the Akkadian equivalent of *si*. From the arrangement of the preserved signs it is clear that the verb *nadû* itself did not fill the line. To all appearances we have, therefore, to restore it to *nadû si* . . . ., which means that *si* is not the equivalent of *nadû* in all meanings of that verb,<sup>11</sup> but only as used in a special sense or in certain phrases.

As already mentioned, however, Thureau-Dangin in his review assumes that the root of *si* — *nadû* is *si(g)* and expressly contrasts it with the root *sum*, *sûn* — *nadānu*.<sup>12</sup> It is not quite clear on what Thureau-Dangin bases this assumption; for, unless I have overlooked some instance in a more recently published text, the only passages in which *si* is equated with *nadû* and at the same time is followed by a syllable showing the last consonant of the root of *si* — *nadû* are the following.

CT 16, 24 f., col. 1, 29 f.:

<sup>20</sup>ki-za-pa-âg-si-mu u-me-ni-dê a-daḥ-zu ḫi-a  
<sup>20</sup>a-dar ri-gim \* na-du-û š-ra-šum-ma lu re-pu-ka

CT 16, 1 ff., 193:

nam-šub-eridu<sup>13</sup>-ga [si]-ma-da-mu-de\*  
 𒌦 šu-pu-er-ri-du<sup>14</sup> ina na-di-a-a 𒌦

<sup>10</sup> This would be possible only in case the line read *na-du-a ša ka-la-ma*; but this is more than unlikely, in view of the fact that the common Sumerian equivalent of *nadû*, as well as the usual Akkadian "ideogram" for *nadû*, is *šub*, not *si*.

<sup>11</sup> Thureau-Dangin: "Il faut lire i-sum et distinguer nettement *si*, *si(g)* — *nadû* de *sum*, *sûn* (dialecte: *zēm*) — *nadānu*."

<sup>12</sup> Var. *ri-gim*.

<sup>13</sup> Var. *si-mu-dê-mu-dê*.



5 R 24 f., col. 4, 6 f.:

91-da-ke	a-na na-a-ru
7ba-ua-si-mu	
	i-na-ad-da-du *

In all of these instances the root of the Sumerian equivalent for Akkadian *nadû* is not *sig*, but *sim*. Obviously, therefore, the *si-i* in the equation

[ *si-i* ] [ (*si*) ] na-da-[u *da* . . . . .]

cannot represent the shortened root *si(g)*, but only the shortened root *si(m)*.

Since thus *si* = *si-i* in the first, second, third, and seventh lines of our syllabary passage represents the root *si(m)*, there can be no doubt whatever that the whole section from line 1 to line 7 treats of this root; and consequently the broken lines 4-6 of the Akkadian column must likewise contain Akkadian equivalents of the root *si(m)*. On the other hand, somewhere after line 7 the syllabary should start to give equations in which *si* represents the root *si(g)*; these would then continue through the rest of the passage, for the root of *si* = *sapânu* in the last equation is undoubtedly *si(g)*, as may be seen from the examples given in Br. 4490.

Since, however, the most common Akkadian equivalent of *sim* is *nadânu*, "to give," and the passage, as we saw, enumerates several verbs of the same meaning in its first section, it is quite certain that line 6 in this first section must be restored as *na-[da-nu]*. It gives us thus a second instance of the equation *si* = *si* = *nadânu*, and moreover leaves no doubt that *si* in this case represents the root *si(m)*.

To sum up, therefore: The pronunciation of *si* = *nadânu* as *si* is attested by two syllabaries (one of the Syll. b and one of the Syll. c class), while that as *sum* is found in only one syllabary (Chicago Syll. = Syll. c class). If, however, we add the various equations of [ *si-i* ] *si* in Syll. c with at least three clear synonyms of *nadânu* and moreover take into consideration that *si* = *nadû* also represents the root *si(m)*, at least six equations attesting the existence of a root form *si(m)* would compare with the one in the Chicago Syllabary for the root form *sum*.

\* Sumerian text has "she will be thrown"; Akkadian, "they (= one) will throw her."

### 3. Evaluation of the Tradition Concerning the Root Forms si(m) and sum

The differences between the two readings of the verb root si = *našánu* in the three syllabaries are the following:

(1). Syll. b, No. 2, and Syll. c give the root without its final *m*, which is amissible and therefore, as in similar cases, can be dropped. The Chicago Syllabary, on the other hand, gives the full root with its final consonant *m*.

(2). Syll. b, No. 2, and Syll. c give the root vowel as *i*, Chicago Syll. as *u*.

#### a. The Vowel of the Root

Disregarding for the present the difference in the treatment of the amissible consonant at the end of the root, or rather treating the final consonant alike in both cases, the difference between the statements of the three syllabaries can be formulated in this way: Syll. b, No. 2, and Syll. c give the root as si(m); Chicago Syll. as su(m).

In order to evaluate this observation properly we have to remember that it is one of the most common features in the Akkadian syllabaries to give different pronunciations of the same word root. Especially frequent is the pronunciation of a root with *i* in one case, and with *u* in another. For instance, Chicago Syll. in lines 92 and 93

227 ki-i		xi		22 (= ki-ku-u)		in-si-tum
227 ku-u		xi		22 (= ki-ku-u)		"

gives the word for "earth," "land," in the forms *ki* and *ku*, although in both cases the word is written with the same sign *xi*. Note especially that these two pronunciations are found here side by side in the very same syllabary. Compare, furthermore, the parallel word forms

imin and imun, "seven"  
ningi and lungi = *sabz*(t)  
šurun and šurin = *as-si-ru*

Finally, we may point to the dialectical interchange of *u* and *i* or *e* in word roots, as, e. g., in

nru(d), ES eri(d), "slave"  
dug, ES seb, "knee"  
elim, ES elum = kabtu  
en (< awan), ES umun (= awun), Ga, "lord"

Regarded in the light of these observations, the fact that the root of the verb for "to give" appears in the syllabaries in two different forms, namely *si(m)* and *su(m)*, is not at all surprising, and this fact alone would in no way justify Thureau-Dangin's assumption that one of the two given pronunciations is due to an error.

Moreover, an explanation for the existence of the pronunciation *sum* beside the pronunciation *sim* may be readily found. For the *u* of *sum* can be due to the influence of the labial consonant *m* on the *i* of *sim*, this vowel being partially assimilated to the *m* by being changed into the labial vowel *u*. This explanation becomes even more likely, if not absolutely certain, when it is observed that the change from *imin* to *umun*, *ningi* to *lumgi*, *elim* to *elum*, and *e(me)n* to *umun* was caused, no doubt, by the very same retrogressive influence of the consonant *m* on the vowels *i* or *e* which precede it. The interchange of *i* and *u* as the vowels of the root *sim* = *nadānu* can thus be explained as a natural phonetic development.

Obviously because of the observation just made *si(m)* would appear to be a more original, *su(m)* a secondary pronunciation of *si*, "to give"; and the fact that in Syll. b, No. 2, and Syll. c it is glossed *si-i*, but in the Chicago Syllabary, *su-um*, would then merely mean that the former give us the older, the latter, a younger pronunciation.

#### b. The Amissible Final Consonant

A similar result will be obtained from observations made concerning the treatment of the final consonant *m* of the root *sim* or *sum* in the two groups of syllabaries. The very fact that Syll. b, No. 2, and Syll. c, No. 1, give the pronunciation of *si* — *nadānu*, the root of which is *sim*, only as *si*, that is, without the amissible final *m* of the root, whereas the Chicago Syllabary gives it with *m*, is itself a proof that the reading given by the former syllabaries is more original than that given by the latter. For in old Sumerian, that is, in the times when Sumerian was a spoken language, the amissible consonant at the end of a word was regularly dropped unless it was protected by a following grammatical element beginning with a vowel (GSG § 39). In post-Sumerian times, however, this rule concerning the dropping of amissible consonants was to a large extent purposely neglected in the schools because instruction in, and the use of, Sumerian became much easier by such simplifica-

tion. This observation applies also to the pronunciation of signs or words, as given in the syllabaries; in other words, those glosses in the syllabaries which do not render the amissible consonant at the end of a root reflect more or less the usage and the pronunciation of the time when Sumerian was a living tongue, while those glosses which pronounce the amissible consonant reflect the usage of the post-Sumerian period. Glosses of the first type are quite frequent in the syllabaries and vocabularies, though far less so than those of the second type.<sup>14</sup> Note, e. g., that the equations Syll. c: DT 40 (= CT 11, 29 ff.), col. 2, 3:

([ si-i ) | si | (su-su-su) | sa-pa-[nu]

and Chicago Syllabary, l. 121:

[ si-i | si | "[= su-su) | sa-pa-nu

gloss not only si — *madanu*, but also si — *sapānu*, "to overcome," as si, although numerous passages, e. g., those quoted in Br. 4420, show quite clearly by the "complements" -ga, -gi, and -ki that the root of the verb si — *sapānu* is sig or sik, not sil. In fact, the value sig postulated for si in former times on account of the fact just mentioned is as yet not given, and probably never was given, in any vocabulary. Similarly in the equation 5 R 31, No. 4, col. 2, 16:

du du<sub>2</sub> | so-a-bu

the Sumerian word for "good" is given as du, although its root, as is well known, is dug.

Especially interesting, however, is the following case. Instead of the equation in CT 11, 23 (= Syll. b, Assyrian), col. 2, 14 (corresponding to K 110, col. 2<sub>1</sub>), and Weissbach, *Miscellen*, Plate 10 f., col. 2<sub>21</sub> (Babylonian duplicate):

[ mu-ru | mu<sub>2</sub> | ga-b-luu, "middle," "midst"

the Babylonian duplicate F. 1, Delitzsch, *Assyr. Lesestücke*, 3rd edition, page 64, l. 9, has the equation:

[ mu-ru-ub | mu<sub>2</sub>ub | ga-ab-lu(!), "middle," "midst"

<sup>14</sup> The preponderance of the second kind of glosses is, of course, due to the fact that the vocabularies at our disposition were written in post-Sumerian times.

That *murru* represents the old Sumerian, *murub* the post-Sumerian, pronunciation becomes quite evident when we compare with each other the phonetic spellings of the phrases, *Gudea*, *Cyl. A*, col. 26, 17:

*mu-ru-dingir-ri-ne-ka*, "among the gods"

and *Rim-Sin*, *Canephore A*, col. 3, 12:

*mu-ru-ub-dingir-gal-gal-e-ne-ta*, "among the great gods."

In this case the shorter pronunciation has maintained itself in the syllabaries for a remarkably long time; in fact, we find the pronunciation *murub* as yet in only one duplicate of *Syll. b*, although in the texts, as we just saw, it was in use as early as the time of *Rim-Sin* of *Larsa*.

No less instructive is the rendering of the word *pa(b)*, "first," "brother," "father," etc., in the various syllabaries. The *Nippur* syllabary, *HGT* 102 (time of the first dynasty of *Babylon*), in col. 4, 4 ff.:

4	pa	PAB	re-bu-a
7		PAB	a-še-ri-du
8		PAB	re-a-du
9		PAB	a-bu-um

gives only the pronunciation *pa*. The younger *Nippur* syllabary, *HGT* 104, on the other hand, gives in col. 3, 24 f.:

7	pa-ab	PAB	a-[...]
9		PAB	a-še-ri-du

only the value *pab*. An intermediary stage is found in the two approximate duplicates of *HGT* 104, namely, *Yale Vocabulary*, II. 222 ff.:

222	pa-a	PAB	" (= pa-ab-bu)	ka-ka-si-ga
	[ <sup>u</sup> ]	PAB	"	a-bu
224	[pa]-ab	PAB	"	a-še-ri-du

and *CT* 35, 1 ff., col. 3, 55 f.:

55	pa-a	PAB	a-bu
	pa-ab	PAB	a-še-ri-du & a-bu

It will be noted that the value *pab* is given only after the shorter value *pa*, in itself an indication that *pa* is the older, *pab* the younger, value. This is definitely proved, moreover, by the observa-

tion that the word pa(h)-gilga, "grandfather," "uncle," literally "the first relation of the father," which is written pa-gi[bi]l-ga and pá-gi[bi]l-ga at the time of Ammiditana (cf. date formula of his 34th year),<sup>11</sup> is written pa-*gîš*-bil-ga in Mannatum, Boulder A, col. 8, 4, and pa-gi[bi]l-ga in Summulum, LIH 98 and 99, l. 64. Compare also pá-šeš = *ašarēdu*, "first," and ahu *rešū*, "first (of the) brother(s)," which passed over into Akkadian as the loan word *pašēu*, "first."

Furthermore, the late Syll. c, No. 1 (CT 11, 29 ff.), col. 4, 33 ff.:

└ ts-ag	TAG	šu-ti-du	zu-u'-u-ne
			la-ba-pu
			ma-ba-pu šd mi-ne
			an-ba-pu š du-a-rum
			šd-a-lum š na-ba-pu
			na-du-u

gives, as pronunciation of TAG in the meanings just enumerated, only tag. AO 7661,<sup>12</sup> which is the immediate continuation of the Chicago Syllabary, in column 2, 15 ff.:

└ ta-ag	TAG	ma-ba-pu š na-tu-u
		na-ka-su š na-ka-pu
		si-nig-tu š hu-ut-tu-tu
		šd šu-TAG-TAG šu-ta-mu-du
		šd šu-TAG-TAG pu-taš-šu-ru
		šd ū-TAG in-gu
		lu-pa-tum šd ka-la-ma
		" šd <i>gîš</i> .šā.ta
└ ta-a	TAG	šuk-lu-lu š šd nam-TAG-ga ā-ru
└ da-a	TAG	šuk-lu-lu š šd nam-TAG-ga ā-ru

likewise gives in its first section the reading tag, but also lists, though only as a sort of addendum, the older values da and tu without the final *g*.<sup>13</sup> Finally, the sign list CT 41, 47, which, with the lists preceding and following it, formed the nucleus of the

<sup>11</sup> See Poebel, *Babylonian Legal and Business Documents* (BEUP, vol. VI 2), pp. 95 f.

<sup>12</sup> Published by Scheil in *Nouveaux vocabulaires babyloniens*, pp. 3 ff., and by Thureau-Dangin in *Tablettes d'Uruk* (TC VI), No. 37.

<sup>13</sup> By this arrangement the syllabary indicates that at his time the generally recognized value of TAG was tag. Ta and da seem to be listed rather as curiosities, referring to old but still known writings or pronunciations.



Chicago Syllabary and AO 7661 and is of a much earlier date than either of them, enumerates in ll. 85 f.

└ ta TAG  
└ da TAG

only the shorter old Sumerian pronunciations *ta* and *da*.

Note, finally, that the same old sign list in l. 87

└ še-ri TAG

gives for TAG only the shorter phonetic value *šeri*, as contrasted with the values *šerim* and *šerid* given by AO 7661, col. 2, 27:

└ še-ri-im | TAG | šá giš-TAG *giš-tum*

and col. 2, 33:

└ še-ri-id | TAG | KA-KA-si-ga

The value *šeri* of the old sign list represents the first part of the root *šerim* as well as the root *šerid*, just as the phonetic value *si* in Syll. c, No. 1, represents the first part of the roots *sim* and *sig*.<sup>24</sup>

There can therefore be no doubt that the pronunciation *si* given by Syll. b, No. 2, and Syll. c: D 40 for *si* = *našim*, as far as the treatment of the final *m* is concerned, reflects an older usage than the rendering in the Chicago Syllabary with *m*, a conclusion that is in complete accord with the fact that the vowel *i* of *si*, as has been shown above, is the original, while the *a* of *sum* is secondary.

### 3. Refutation of the Arguments for the Exclusive Reading *sum*

#### a. Doubling of the Final Consonant

Having noted these findings, which completely reverse Thureau-Dangin's evaluation of the statements in the syllabaries, we are now prepared to examine the validity of the reasons adduced by him in support of his opinion.

We begin, then, with Thureau-Dangin's general view of the admissibility of final consonants in Sumerian. As far as I know, Thureau-Dangin has never published a comprehensive statement on

<sup>24</sup> The values *šerim* and *šerid* seem to have some connection with the Akkadian equivalents of *taš* in the equations └ šu-uš | TAG | . . . , *šé-ra-mu*, col. 2, 30, and *nam-tag-ga* = *šertum*, Del., HW, p. 626; but owing to the deficiency of the material nothing certain can be concluded.

this question, but he has explained his stand in a letter to the writer, dated August 8, 1928, whose substance is as follows. He recognizes the amissibility of consonants in Sumerian, but believes it necessary that in every special case it should be expressly attested by Babylonian vocabularies, sign lists, glosses in texts, etc., since it is likely or at least possible that certain Sumerian roots end with a double consonant and that such roots probably do not drop both of the two final consonants. To illustrate this point he refers to the French *si* from Latin *sic*, *ami* from *amicus*, and *épi* from *epicus*; but *bec* from Latin *becum*, *sac* from *sacrum*, and *sec* from *siccum*. In the first group the *c* is dropped because the root of the Latin original ended with *one c* only; in the second group, however, the *c* is preserved and pronounced because the root of the Latin original ended with *double c*. In his letter Thureau-Dangin restricts his remarks to words ending with *k*; he therefore had no occasion to mention the root *sum*, "to give." But his view would affect this root, because its Emesal form *zem* without any exception doubles the final *m* before vowels; cf., e. g., *mudru* (?) -*zé-em-mu* = *na-din kat-ti* (4 R 9, obv. 34 f. [Haupt, *Die akkadische Sprache*, XXXIX]) and *nu-zé-em-má* = *u! a-nam-[din]* (ASK II, No. 21, rev. 27 f.). Thureau-Dangin, in fact, might even use this Emesal form *zem* as a very welcome illustration of the rule suggested by him in his letter, because the *m* of *zem* in the cases where it stands at the end of the word is never dropped; cf., e. g., *ma-an!-ze-em* | *ma-an-si* | *id-di-[na]* (5 R 12, No. 1, last column, l. 25).

As early as twenty-five years ago, when I began my collection of words for a Sumerian dictionary, I was obliged to test the very possibility suggested by Thureau-Dangin in his letter. In fact, at that time it seemed to me an axiom that those roots which in the transliteration of Sumerian signs then commonly accepted seemed to be written with a double consonant, e. g., *gal* in *gal-la* and *dingir* in *dingir-ra*, should be listed with a double consonant at the end, i. e., e. g., *gal* as *gal(l)*, *dingir* as *dingir(r)*. But I soon became convinced that it is not possible to prove with any degree of certainty for any Sumerian root that the double consonant found at its end is actually one of its original, and not merely accidental, features. Nor was it possible to prove that the other roots, including even those whose vowel originated from a contraction of two vowels, could not at times appear with doubled final

consonant, at least when using the commonly accepted system of transliteration based on the phonetic values attributed to the Sumerian signs by scribes of the post-Sumerian period. On the contrary, all observations tend to show that a consonant can be doubled or sharpened whenever it follows a stressed vowel and is followed by another vowel. Note, e.g., that the loan word *ekil* (from Akk. *eqle*) is written *ek-kil* in the Chicago Syllabary, l. 280, and that the old Sumerian prefix forms *eme-* (or *imi-*) and *ema-* appear as *immi-* or *imma-*, respectively, in late Sumerian and post-Sumerian texts. This observation holds good even for the very latest post-Sumerian periods, as may be seen, e.g., from the fact that *(giš)sa<sub>4</sub>* in Syll. b, No. 2 (= CT 11, 35 ff.), col. 3, 23, is glossed *gi-ši-im-mar*, but in PSBA 1902, p. 109, 1 f., which dates from the Seleucid period, is rendered as *γισμαρ*, with one *μ* only. All our observations tend to indicate, moreover, that every vowel of a Sumerian root is basically short, apart, of course, from those roots in which a long vowel has resulted from the contraction of two short vowels.<sup>10</sup> Theoretically, therefore, the final consonant of every Sumerian root could be doubled, although, as we shall see, there could also occur the lengthening of the short vowel.

The gathering of evidence in the matter is rather difficult because in the old Sumerian system of writing neither the doubling of a consonant nor the length of a vowel is expressed. But it must be realized that this peculiarity of the Sumerian system of writing, which also passed over into the older Akkadian system (and can even be traced down to the older alphabetic systems), is natural only if the language by which that system was used did not itself distinguish between short and long vowels nor between single and double consonants. The very fact that the Sumerian system of writing shows that peculiarity may therefore be taken as a clear indication that Sumerian actually had no roots with final double consonant or with a basically long vowel.

The same result can be obtained from an investigation of the

<sup>10</sup> Cf. *ēn* (< \**ewen*) = *ESumun*, *un*, *ū(n)*, "divine lord," *sūn* < *sumun* (*suwun*), "old," *dū* < *dumun*, "child," *nēn* < *nimin*, "to go or stroll about," *ninā* (vocabulary gloss *nī-na-a*, CT 11, 35 f.: S 1300, col. 3, 0; *nī-nā-a*, Chic. Syll. 150) and *ninua* (gloss *nī-nū-a*, Chic. Syll. 150); cf. *Chuziane* < \**sinuwan* (preserved in Hebr. חֲזִיָּא), Greek *zēueu*, *zēue*, and Arab. *يَتَوَى*. Assyrian *nīnuu* < \**nīnuwan* like *tudra* < \**tawdra*, *itaw* < \**itawar*.

treatment of the vowels and the final consonants in loan words from Sumerian in Akkadian, especially in the names of cuneiform signs, which are formed according to the same rules as those applying to the former. Most of these sign names double the final consonant of the corresponding Sumerian phonetic value of the sign; compare, e. g., *dibbu* (written *dib-bu*)<sup>16</sup> for the sign *PIB*, and *tukullum* (written *tu-kul-lum*)<sup>17</sup> for the sign *TUKUL*. But a number of sign names lengthen the vowel of the Sumerian word instead; compare, e. g., *dûgu* (written *du-u-gu*)<sup>17</sup> for *DUG*, and *mâsu* (written *ma-a-su*)<sup>18</sup> for *MAR*. Yet from the writing of the Emesal root *zeb*, "good," which is etymologically identical with the root *dug* of the main dialect, as *zebb* in *zé-ib-ba* — *ja-a-bu* (K 101, Obv., l. 11), *a-ze-ib-ba* | *a-du<sub>11</sub>-ga* | *mé<sup>21</sup> ja-bu-tu* (5 R 11, Col. 2, 27), and *su-nu-zé-ib-ba* — *la ju-ub šî-ri* (Langdon, BPP, pl. 2: K 4664, ll. 8 f.), it is evident that the *e* of the Sumerian root *dug*, "good," was not long, or at least not basically long. Clearly, the lengthening of the vowel or the doubling of the final consonant in the name of a sign is in no way an indication that the vowel of the corresponding Sumerian root is long or short, nor does it prove whether the root ends with a simple or a doubled consonant. The very fact, however, that the Akkadian sign name either lengthens the vowel of a Sumerian root or doubles the final consonant is a sure indication that the vowels of all Sumerian roots are basically short, lengthening of the vowel as well as doubling of the following consonant being due simply to the influence of accent.

Above, we have briefly referred to the fact that the old Akkadian system of writing, which is based on the Sumerian and is essentially identical with it as far as its basic principles are concerned, agrees with the Sumerian system also in the principle of leaving length of vowel and doubling or sharpening of consonant unexpressed. Since in Akkadian, as in all other Semitic languages, length of vowels was a very essential means of expressing grammatically important differences, it would have been natural for it to invent from the start means of expressing the auditory differences in writing also. Quite on the contrary, however, even those phonetic values of the Akkadian system which were derived from genuine Akkadian words are used with complete indifference as to length of the vowel. The value

<sup>16</sup> CT 11, 1 f., col. 3, 34.

<sup>17</sup> Yale Syll., l. 113.

<sup>17</sup> CT 11, 1 ff., col. 2, 11-14.

<sup>18</sup> Yale Syll., l. 243.

*bīt*, e. g., for *ē*, derived from Akkadian *bītum*, "house," and the value *ram* for *ān* (instead of *ni-ān*), derived from *rāmu*, "to love," are used to render not only the syllables *bīt* and *rām*, but also the syllables *bī* and *rā*. On the other hand the value *amat* for *am*, derived from *amum*, estr. *amat*, "maid," "girl slave," and the value *šam* derived from *šammu*, "herb," in spite of the short vowels of their prototypes are employed also for *āmat* (e. g., in writing *tāmat* as *tī-amat*) and *šam*. The principle underlying this remarkable usage is simply inherited from the Sumerians, who had adopted it because their language did not possess word roots with basically long vowels.

This Sumerian legacy, by the way, extended even into the Akkadian language. Among all Semitic languages, Akkadian alone shows a marked propensity to double the consonant after a stressed short vowel, at least in the forms of its present tense, as, e. g., in *ikāššad* < *ikāšād*. This is simply due to the influence of the Sumerian language, in which as a rule the consonant could be doubled after a stressed short vowel. Even more significant is the fact that among the Semitic languages Akkadian alone, to any rather large extent, resorts to replacing a phonetic complex consisting of long vowel and consonant by a complex consisting of short vowel and double consonant, or vice versa. Compare, for instance, *multerru* < *mulēru* (< *mulairu* < *mulayirum* < *mulayyirum*); *turru* < *tāru* (< *taurum* < *tayyurum*); *habullu* < *\*habālu*; *šūmu* < *šummu* (< *šun'u*); *bīru* < *\*birru* (< *bir'u* < *birju*). This interchange clearly reflects the fact that the Sumerian word roots with their basically short vowels can lengthen the vowel or double the following consonant. These observations, however, again are a valuable corroboration for our conclusions concerning the character of the vowels and the rôle of the doubling of consonants in Sumerian.

Returning now to the question raised by Thureau-Dangin with regard to the amissibility or non-amissibility of the final consonant of a Sumerian root, in the light of the results just obtained we can now state that there are no Sumerian roots which are characterized by final double consonant; such a feature, therefore, cannot be a criterion for the inamissibility of that consonant. Doubling of the final consonant is frequently found in the usual Emesal texts, which favor the broken writing of closed syllables and therefore can unequivocally indicate the doubling of a consonant, whereas the

so-called "ideographic" writing favored in the main dialect is always ambiguous as to whether the "ideogram" should be read with a shorter value—corresponding to the older usage—or a longer value—corresponding to the later custom. But the usual Emesal texts themselves belong in the period characterized by the tendency to restore the formerly dropped consonants at the ends of words. And even if the presence of the final amissible consonant at the ends of words in Emesal should be due to the fact that this dialect never went as far as the main dialect in dropping final consonants, that is, it preserved the final consonant in certain cases, this would prove nothing for the main dialect, to which both the root forms *si(m)* and *sum*—the subjects of this study—belong.

Nor, in the light of the results obtained in our investigation, can the doubling of the final consonant of the Sumerian root in the comparatively late Akkadian and Sumerian sign names be considered indicative of the inamissibility of that final consonant. This point can be established directly from statements of the syllabaries, etc. The sign *pappu* (written *pa-ap-pu*, Yale Syll., II. 220 ff.), for instance, is not only given the phonetic value *pab* with preserved *b* at the end, but also, as we saw, the value *pa* without that *b*. Indeed, the texts show that throughout the older period the sign is read *pa*, and that only in later, that is, post-Sumerian, times, is the pronunciation *pab* given. In the same way the sign *siggu* (written *si-iq-qu*, CT 11, 1 ff., col. 5, 49 f.) has not only the value *sig* in Akkadian (*šig*), but also *se<sub>11</sub>* (HGT, No. 124, 3) and (in the old Akkadian) *ši*. Note, furthermore, the values *gu<sub>4</sub>* and *gud* of the sign *gu<sub>4</sub>gu* (written *gu-ut-gu*, Br. 5731), *la* and *lal* for *lallu* (written *la-al-lu*, Yale Syll., 207 f.), *zā* and *zag* for *zaggi* (written *za-zg-gu*, Br. 6469), *gu<sub>2</sub>* and *qam* for *qamnu* (written *gu-am-[nu]*, CT 11, 13: 46387, col. 3<sub>30</sub>), *uri* and *uru* for *urina* (written *u-ri-in-na*, CT 11, 29: K 7668, 1-5), and *teme*, *temen*<sup>10</sup> for the Akkadian loan word *temennu*.

Returning to the verb *si*, "to give," we can now say, on the basis of the foregoing observations, that neither the doubling of the final *m* of the root *zem* in the forms already mentioned above, nor the doubling of the final *n* in the sign name *sumnu* (written *su-un-nu*

<sup>10</sup> The strange value *timmena*, which is given by Syll. b (CT 11, 14 ff.), col. 5, 43, is probably a loan word from the Akkadian *temennu*, which is itself a loan word from Sumerian.

in CT ii, 29 ff., col. 7, 80 and col. 2, 1, derived from a value *sum* which Thureau-Dangin thinks is identical with our word *sum*), can in any way prove that the final *m* of the root was inamissible in the main dialect during the older period. In Emsal, it is true, the *m* of the root *zem*, "to give," is treated as an inamissible consonant, as is shown, e. g., by the verbal form *ma-an( ! )-ze-em*, "he has given to me" (5 R 12, No. 1, last column, l. 35). Similarly in the post-Sumerian period the *m* of the root *sim* of the main dialect could be treated as inamissible, as is proved by the phonetic value *sum* given in the Chicago Syllabary. At that time, indeed, there is no doubt that some of the scribes were accustomed to pronounce the verbal form *ma-an-si*, "he has given me," exclusively as *ma-an-sum*. But, as we saw, even very late syllabaries still give the old pronunciation *si* for *si*, "to give," which indicates that even in the latest times some Akkadian scribes or certain schools pronounced the root *si*, "to give," *si(m)*. This fact, as well as the reading *si* for *si*, "to give," in the old Sumerian period, therefore, is in no way affected by the doubling of the *m* in Emsal and the doubling of the *n* in the sign name *sumnu*.

#### b. The Vowel *u* after the Root

Since Thureau-Dangin does not indicate the details of his argument that the reading *sum* for *si* — *nadānu* is proved by the phonetic "complement" —*mu*, we are left in uncertainty as to whether he wants to base his argument exclusively on the consonant *m* of the "complement" or also on its vowel *u*. If we assume the former to be the case, his argument would evidently run as follows. As indicated by the *m* at the beginning of its "complement" —*mu*, the root of *si* — *nadānu* must doubtlessly end with an *m*. Now since *sum* is the only value ending with an *m* that is given as a reading of the root *si* — *nadānu* in the syllabaries, this value *sum* must be considered to be the only possible reading of the root for "to give" to the exclusion of, for example, a reading *sim*. As a matter of fact, however, the argument is conclusive only as far as the final consonant *m* of the root is concerned; it does not prove that the vowel of the root was necessarily and exclusively an *u*. For the fact that up to the present only *sum* is found in a syllabary as a value of *si* with final *m* may of course be quite accidental; that is, it is not at all inconceivable that some new syllabary will give us a value *sim* for *si* — *nadānu*. In order to assume, against the



testimony of two syllabaries, that a value with the vowel *u* is the only correct reading, it would be necessary to adduce additional evidence which proves conclusively that the vowel of the root of *sîm* — *wadânu* can be nothing else but an *u*. It is therefore quite possible that Thureau-Dangin did not intend to stop in his argument at the point just indicated, but that he may actually have had in mind to prove the *u* as the only possible vowel of the root for *sî* — *wadânu* from the vowel *u* of the "complement" -*mu*. But whatever Thureau-Dangin's reasoning in this case actually was, it will be necessary for us to investigate the question whether the *u* found so frequently after the root *sîm* is really an indication that the root must be *sum*.

The *u* found after the root *sîm*, as, e. g., in ASK II, No. 1, (*Aaa ittîšu*, Tablet I), col. 1, in the well-known verbal forms

in-sî-mu	he will give	l. 10
in-sî-mu-ne	they will give	l. 20
in-sî-mu-uš	they have given	l. 18

as well as in *sî-mu-dam*, translated *i-na-aš-din* in ASK II, No. 2 (*Aaa ittîšu*, Tablet II), col. 1; 47, is for the usual *e*, as it is found, e. g., in the following forms of the verb *lal*, "to weigh," "to pay," in ASK II, No. 1, col. 2:

in-lal-e	he will pay	l. 3
in-lal-e-ne	they will pay	l. 4
in-lal-eš	they have paid	l. 2

as well as in *tal-lî-dam*, translated *i-a-ša-ap* (ASK II, No. 2, col. 1, 45). It must be conceded that, in a number of other cases in which the *e* of the grammatical elements following the root changes to *u*, the vowel of that root is indeed an *u*. Compare, e. g.:

*nam-ba-gub-bu-de* = *la ta-at-ta-nam-za-az* (CT 16, 27 ff., ll. 82 and 84)

*i-sub-sub-bu-uš-šim* = *up-ta-az-si-su-ma* (King, LIH, No. 99 and 97 f., col. 3, l. 69 [95])

*mu-ua-dûb-dûb-bu-ne* = *i-nap-pa-gu* (CT 17, 31 f. Obv. 10)

*i-dub-dub-bu* = *ta-bi-ik* [4 R 24, No. 1 Obv. 30 f.]

*tûm-tûm-mu-dê* = [ur]-ru-aw (LIH, No. 98 f. and No. 97, Winkler, AK 74, l. 28 [20, 27, and 23 resp.])

*ba-gul-lu-uš-a*, "which had been destroyed," date formula of the 17th year of Samsuiluna (BE VI, 2, pp. 74 f.)

*tûš-û-dê*, "to cause to dwell," VAT 2684 (unpublished inscription of Samsuiluna) [now published AOI IX, p. 241 ff.]

*nam-ba-gur-ru-da* = *a-a i-ta-ru-ni* (CT 16, 12 ff., col. 4, 36)

ba-an-gár-ru-uš — *ik-taš-šu-uš* (Haupt ASK II, No. 10, rev., ll. 20 f.)  
 ih-da-ab-kúr-ru-a, "who alters" (R-S-S-b, l. 45)  
 gu-ru-uš bi-in-bu-ru-uš = *ig-gu-gu-ma* (CT 16, 12 E., col. 1, 2 and 11)

On account of the comparatively great number of these cases I was myself inclined in GSG § 18 to find the determining factor of the change of the post-radical  $s$  to  $u$  in the  $u$  of the root. However, there are certain roots, e. g., *tar* and *bad* (see below), whose vowel is not an  $u$  but an  $a$ , and after which nevertheless the  $s$  changes to  $u$ . In these cases, of course, the rule would not hold. Far more important, however, is the following consideration:

As may be seen from GSG § 17 f. and my study on The Sumerian Prefix Forms  $s$ - and  $i$ - in the Time of the Earlier Princes of Lagash, assimilation of one vowel to another in Sumerian is usually progressive, that is, the vowel which undergoes the change is assimilated to that of the immediately following syllable.<sup>20</sup> If the second  $u$  in such complexes as *gubb-u* and *tumm-u* (instead of *gubb-e* and *tumm-e*) should be due to the influence of the  $u$  of the roots *gub*, *tum*, etc., the assimilation would in this case be retrogressive, that is, the vowel would have been assimilated to that of the preceding syllable. For this reason the correctness of the conclusion that it is the influence of the  $u$  of the root which causes the change of a following  $s$  to  $u$ , must appear rather doubtful.

Of the eleven verbs enumerated above, four—*gub*, *šub*, *dúb*, and *dub*—end with a  $b$ ; one—*tum*—with an  $m$ ; one—*gal*—with an  $l$ ; one—*tuš*—with a  $š$ ; and four—*gur*, *gúr*, *kúr*, and *bur*—with an  $r$ . The  $b$  and the  $m$  are, of course, labials and can doubtless change an immediately following  $s$  or  $i$  into the labial vowel  $u$ . But  $l$  and  $š$  also are, or can be, of a more or less labial character, as is seen from the change of *ištu* (> *\*iltu*, *\*uštu*) to *ultu* in Akkadian. The  $r$  must then be assumed to be of a similar character. It would, naturally, be an  $r$  which, like the labial  $l$  and  $š$ , was pronounced with the lips somewhat pursed. The changing of the  $s$  or  $i$  after the roots enumerated above may therefore with good reason be attributed to the influence of the consonants  $b$ ,  $m$ ,  $š$ ,  $l$ , and  $r$  at the end of the verbal root instead of the vowel  $u$  in the middle of the root. This would also offer a satisfactory ex-

<sup>20</sup> For the comparatively rare retrogressive assimilation of a vowel cf., e. g., the change of *da* and *ra* to *de* and *re* after  $e$  in the infixes *-e-de* and *-e-re* (see GSG § 13).

planation for the fact already mentioned that we find the *u* after roots ending with *r* but not containing the vowel *u*. Compare *nam ha-ba-ra-tar-ru-da* = *li-ru-ru-sá* (CT 17, 34 ff., 39 f.); *mu-ru-na-an-tar-tar-ru-da* = *u-par-ri-t* (4 R, 7 f., col. 3, 40 f.); *he-mi-ib-tar-r[u-ne]* = *li-gaz-si-[su-sá(?)]* (CT 17, 34 ff., 65 f.); note, furthermore, the form *ha-an-bad-du-uš* = *is-su-ú* (CT 16, 42 ff., 68, variant), which indicates that in Sumerian there even existed a *d* which was pronounced in such a way that it could influence the following *e* to become an *u* (or, as in this case for certain reasons is more likely, an *o*).<sup>21</sup>

As far as merely phonetic conditions are concerned, we may also quote, in support of the suggested influence of a labial or another consonant on the following vowel, the readings of the following nouns and verb roots:

- šanabi*, *sunabu*,<sup>22</sup> "two-thirds"  
*lím*,<sup>23</sup> *lam*,<sup>24</sup> *limma*, *lammu*, "four"  
*elam*, *elamu*,<sup>25</sup> "Elam"  
*nammu*, "Nammu"  
*šasari*, *šasaru*,<sup>26</sup> "Marduk"  
*gabari*,<sup>27</sup> *gabaru*,<sup>28</sup> "equal," "rival"  
*dari*,<sup>29</sup> *daru* "  
*gištar*,<sup>30</sup> *gištur*,<sup>31</sup> *gištur*,<sup>32</sup> *gešdaru*,<sup>33</sup> *gešturu*,<sup>34</sup> *gidri*,<sup>35</sup>

<sup>21</sup> Compare also the imperative and infinitive forms *bad-du had-du* = *i-si ri-e-ke* (Kbeling, KARI, No. 31, rev., 3 f.) and *ka-bad-du | pi-it pi-it* (CT 12, 38: K 244, col. 1, 8); further *u-sá-du*, "distant days" (CT 15, 17); *sū-du-sē*, "into the distance," "unto distant days" (Zimmern, SK, No. 189, col. 1, 9, 13), etc.

<sup>22</sup> Compare *su-ur-su-na-bu*, "Šur-Sunābu," name of the boatman of Utanapištim in the Old Babylonian version of the Gilgamesh Epic, Tablet X, = *zu(r)-šanabi*, "Zu(r)-Šandbi" in the Assyrian version. The reading *zu(r)* of the *un-* of proper names (cf. *un-annāne*, *un-annammu*, etc.) will be treated in a forthcoming study.

<sup>23</sup> See Thureau-Dangin, HS, p. 20, n. 13.

<sup>24</sup> Syll. a (CT 11, 1 ff.), col. 6, 13 (variant *elam*).

<sup>25</sup> Gloss *a-sa-ru* to *annan-ri*, Br. 924.

<sup>26</sup> Written *gaba-ri*.

<sup>27</sup> Written *gaba-ru*, HGT, No. 34, col. 5, 14.

<sup>28</sup> AO 8970 (RA XXI, p. 170), col. 2, 21.

<sup>29</sup> Chicago Syll. 203.

<sup>30</sup> Cf. the Sumerian phonetic value *giš-tar*, *gi-iš-tar* in various sign names, Br. 5559.

<sup>31</sup> Cf. the Sumerian sign name *giš-tu-ra* (= *gištar-a*), CT 11, 27: 81-7-27. 209 (duplicate of Chicago Syll.), rev. 17.

<sup>32</sup> Cf. the Sumerian sign name *giš-tu-ra* (= *gištar-a*), Chicago Syll. 225.

gidru,<sup>36</sup> ES mudru,<sup>37</sup> muduru<sup>38</sup>  
 zigara, zikara, zigaru, "heaven," "sky"<sup>39</sup>  
 eš,<sup>40</sup> eššu,<sup>41</sup> "three"  
 ūš,<sup>42</sup> ūšu,<sup>43</sup> "dead"  
 anše,<sup>44</sup> anši,<sup>45</sup> anšu,<sup>46</sup> "donkey"  
 i<sub>12</sub>[d],<sup>47</sup> iti,<sup>48</sup> itu;<sup>49</sup> i<sub>17</sub>[d],<sup>50</sup> iti,<sup>51</sup> ita,<sup>52</sup> "month"  
 midda,<sup>53</sup> miṭṭu,<sup>54</sup> "weapon"  
 baḍ,<sup>55</sup> buṭṭu,<sup>56</sup> "to open" etc.  
 [giš-]essaḍ,<sup>57</sup> (giš-)essaḍu,<sup>58</sup> "snare," "trap"<sup>59</sup>

<sup>36</sup> Cf. the Akkadian sign name *gi-ā-da-ra-s* (< *gisdara-u(m)*), Br. 5550.

<sup>37</sup> Cf. the Akkadian sign name *gi-ā-ṭu-ru-u* (< *gisṭuru-u(m)*), Yale Syll. 253.

<sup>38</sup> CT 35, 1 ff. (duplicate of Yale Syll.), col. 4, 21 (*gi-id-ri*).

<sup>39</sup> Yale Syll. 268 (*gi-id-ru*).

<sup>40</sup> Yale Syll. 269 (*mu-ud-ra*).

<sup>41</sup> Written *mu-du-ru*, M 740, Delitzsch, *Glossar*, p. 96.

<sup>42</sup> Delitzsch, *Glossar*, p. 225.

<sup>43</sup> CT 35, 1 ff. (duplicate of Yale Syll.), col. 4, 27.

<sup>44</sup> Yale Syll. 275.

<sup>45</sup> HQT, No. 102, col. 5, 6.

<sup>46</sup> Syll. a (CT 1, 1 ff.), col. 5, 21.

<sup>47</sup> CT 12, 31: 31177, ll. 34.

<sup>48</sup> Syll. a (CT 11, 1 ff.), col. 4, 10.

<sup>49</sup> Ibid., col. 4, 10, and variant of l. 18.

<sup>50</sup> Cf. the frequent *i<sub>12</sub>-da*, "per month," and the Sumerian sign name *ita* (= *it-a*) in the sign name *ša-i-tak-ku-baḍ-i-du*, Syll. b (CT 11, 14 ff.), col. 2, 20.

<sup>51</sup> Syll. b, *Pragm. F 1* (Delitzsch, *AL*, 3<sup>rd</sup> ed., p. 64), l. 7.

<sup>52</sup> Syll. b (CT 11, 14 ff.), col. 2, 19.

<sup>53</sup> Cf. n. 9.

<sup>54</sup> Syll. b, *Pragm. F 1* (Delitzsch, *AL*, 3<sup>rd</sup> ed., p. 64), l. 8.

<sup>55</sup> Syll. b (CT 11, 14 ff.), col. 2, 26.

<sup>56</sup> Yale Syll. 136 (*mi-id-da*).

<sup>57</sup> CT 35, 1 ff. (dupl. of Yale Syll.), col. 2, 39 (*mi-iṭ-ṭu*).

<sup>58</sup> HQT, No. 102, col. 5, 15.

<sup>59</sup> Syll. a (CT 11, 1 ff.), col. 5, 32 (*ba-aṭ-ṭu*).

<sup>60</sup> CT 17, 25 f., obv., 14, written ([giš-])ēš-sa-aḍ (= *naḫḫaḫ*); CT 19, 48 f., col. 2, 16, gloss *ēš-sa-aḍ* for *zaq-ḫa* (= *iṣḫu id* [*nāni*]).

<sup>61</sup> CT 13, 43 ff., col. 1, 56, written *ēš-sa-du* (= *naḫḫaḫ*); 2 R 22, No. 1, col. 1, 29, written *giš-ēš-sa-du* (= *naḫḫaḫ*).

<sup>62</sup> In Urukagina, Cones A and B (and † duplicate cone C), and Gudea, Cyl. B, *essaḍ* denotes a person: "catcher," "taker," "hunter" = *ḫa-biḫ*, synonym of *saḫḫaḫ*, Gilg. Epic, Tabl. I. Note that the accusative in Urukagina, Cones B and C, col. 8, 22, is written *zaq-na* = *essa(d)*, while the subject case in Cones B and C, col. 3, 22, and Cone A, col. 4, 9, appears as *zaq-na-du* = *essa-du* (for *essaḍ-e*); here, therefore, the -u represents the grammatical element -e, which has been changed into -u by the preceding d. In the 4-lemma-*essaḍ-e-gá-edin-na* of Gudea, Gl. B, col.

[u(d)],<sup>12</sup> uta,<sup>13</sup> utu,<sup>14</sup> "sun"  
 [d̄m̄ta(t)],<sup>15</sup> d̄m̄tati,<sup>16</sup> d̄m̄tatu,<sup>17</sup> "Nabû"  
 idigla,<sup>18</sup> idiglaa,<sup>19</sup> idiglu,<sup>20</sup> "Tigris."

Almost all of the words just enumerated have, or seem to presuppose, a root without a final vowel. This vowel seems to be of secondary character (see GSG § 89) and therefore must be short and changeable. Furthermore, the final consonants of these shorter roots are *b*, *m*, *r*, *š*, and *d/t*, that is, the very same labial or related consonants which, as was shown above, were characterized by a propensity for changing a following *s* to *u*. Even the *s* of Sumerian *idiglu* must have been of a similar character, since in the Akkadian (*i*) *diglat* it appears as *l*, and in Greek *τιγρις* as *r*. It seems therefore quite plausible to assume that the final *u* of the words listed above is likewise due to the influence of the preceding consonant.<sup>21</sup> The forms *limmu* and *lammu* should be especially noted because they have an *u* after *m*, although the vowel of the root itself is *i* or *a*, not *u*. This would be a case completely analogous to the combination *si(m)mu* = *si-mu*. Note furthermore that the root *limm(u)*, "four," in a late period occasionally seems also to appear as *lumm(u)*,<sup>22</sup> which fact again would be parallel to the appearance of the root *sim* as *sum* in late times.

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12, *š*, the *s* obviously does not belong to *essad*; the phrase should be translated: "the *essad* of the canals and of the Guedinna."

<sup>13</sup> Usually used only for "day."

<sup>14</sup> Syll. A (CT 11, 1 ff.), col. 2, 61 (*u-ta*); cf. also Thureau-Dangin, *LC*, p. 68.

<sup>15</sup> CT 25, 25, l. 4 (*š-tā* for *šur*); CT 12, 1 ff., col. 3, 41 (*š-tu* for *šx*).

<sup>16</sup> Yale Syll. 202 (*mu-š-a*, there given for *ša* = *štu* only).

<sup>17</sup> CT 35, 1 ff. (duplicate of Yale Syll.), col. 4, 16 (*mu-š-a-ti*).

<sup>18</sup> Yale Syll. 203 (*mu-š-a-tā*).

<sup>19</sup> Syll. b (CT 11, 14 ff.), col. 6, 37, variant (*i-di-gin*).

<sup>20</sup> Syll. b (CT 11, 14 ff.), col. 6, 37 (*i-di-ig-na*).

<sup>21</sup> CT 12, 28, rev. 31 (*i-di-ig-nu*).

<sup>22</sup> It is at present, of course, quite impossible to form these observations into a grammatical rule. For that would require a much more detailed investigation and one which would distinguish between the various periods, the various dialects, peculiarities of the various post-Sumerian schools, the various systems of writing, etc. An important question that ought also to be taken into account is how far the change of vowels at the end of Sumerian word roots was influenced by the fact that in the latest periods the endings of Akkadian words were no longer pronounced and therefore were used almost promiscuously.

<sup>23</sup> Cf. *šūal-šū-lum-ma* = *ti-te-ma* and *ra-i-i-su*, AO 8870 (RA XXI, p.

Taking all this evidence into account, therefore, we may conclude that the change of an  $s$  into  $w$  after certain roots is not caused by the influence of an  $u$  contained in the root, but by the labial character of the immediately preceding consonant at the end of the root. Indeed, the  $u$  which in many of the words quoted above represents the root vowel will probably itself—at least in some instances—be of secondary origin and due to the influence of the following consonant. This latter then would have exerted its influence not only on the vowel immediately following, but also on the one immediately preceding.

If we now apply these observations to the root  $sim$ , it follows that here too the change of the postradical  $s$  to  $w$ , obviously, is not due to the influence of an  $u$  contained in the root  $sim$ , but is caused by the final consonant  $m$  of the root  $sim$ . Similarly the fact that in late times the root  $sim$  shows an  $u$  instead of an  $i$  as root vowel must be ascribed to the influence of this  $m$ , the assimilating power of the  $m$  thus affecting the immediately preceding as well as the immediately following vowel.

In this connection it is quite interesting to note that Cylinder B of Gudea gives the future infinitive form of  $sim$  in col. 6, 14 and 25 as  $si-mu-da$ , but in col. 12, 9, as  $si-ma-da$ . The  $a$  of the latter form is the result of the usual tendency toward vowel harmony which is effected by a progressive assimilation; that is, the vowel  $e$  of  $*sim-ed-a$  assimilates itself to the  $a$  of the following syllable  $da$ . In  $simuda$ , however, this tendency of the Sumerian language has not become effective, and the  $e$  was therefore in a position to be influenced by the preceding consonant  $m$ . Similarly we find an  $u$  in  $tum-mu-da$  (col. 9, 20), and simple  $i$  or  $e$  in  $gi-ni-da$  (col. 6, 12),  $du_{11}-gi-da$  (col. 6, 13),  $pa-dè-da$  (col. 6, 20),  $sikil-e-da$  (col. 6, 24, and col. 9, 6),  $ka-gi-da$  (col. 9, 6), and  $nim-e-da$  (col. 12, 12). Note especially the forms  $du_{11}-gi-da$  and  $ka-gi-da$ , which preserve the  $i$  despite the fact that the roots contain the vowel  $u$ . This fact again shows that the  $u$  in  $si-mu-da$  and  $tum-mu-da$  is caused not by the  $u$  of the root but by the final  $m$  of these roots. The great majority of future infinitive forms in Cylinder B, however, change the  $e$  to  $a$  by assimilating it to the  $a$  of the following syllable  $da$ . Note especially  $gar-gar-ra-da$

140), col. 3, 12 f. It is preceded by  $sišal-xù-1 = ra'ci-xu$ ,  $sišal-xù-2 = šù š-to šu-pa-a-šù$ , and  $sišal-xù-3 = šù š-to "$  (=  $šim-ma-a-šù$ ).

(col. 11, 23 and col. 15, 4), gub-ba-da (col. 15, 1, and col. 15, 11), and gub-gub-ba-da (col. 15, 8), the roots of which have an *u* as root vowel and furthermore end with one of the labial or related consonants. It will be observed that with the exception of *sim-e-da* (col. 12, 12) all of the forms with *e* and *u* occur in col. 6, 11-10, 2, that is, in the first part of the section, col. 6, 11-col. 16, 2, which contains the future infinitive forms.<sup>71</sup> We may conclude from this either that the two sections, col. 6, 11-col. 10, 2, and col. 10, 2-col. 16, 2, were written by different scribes, who followed different grammatical rules, or that, if both were actually written by the same scribe, he followed in the later part of the whole section other rules concerning vowel harmony than in the first part.

For all these reasons the argument that on account of the *u* so frequently found immediately after the root *sim*, "to give," this root must be *sum* and cannot be *sîm*, is inconclusive.

### c. The Sign Name *sunnu*

Thureau-Dangin assumes as root form of *sî*, "to give," not only *sum*, but also *sûn*. Although in his note in *AJSL* 49 he gives no detailed reasons for this opinion, it may be assumed that at least one of his reasons is the fact that the Akkadian name of the sign is *sunnu* (written *su-nu-nu*, CT XI 29 ff., col. 2, l. 3, and *su-nu*, Chic. Syll. 120); and that this name presupposes a Sumerian phonetic value *sûn* for the sign *sî*. This fact alone, however, is by no means sufficient proof for the assumption that this phonetic value of *sî* represents a second root form of the verb *sî* = *nađûnu*. The proof would be complete only if, e. g., in a text passage where there is no doubt that the verb *sî* means "to give," this verb is followed by a "complement" beginning with *n*. I myself know of only one instance where the sign *sî* is followed by a syllable beginning with *n*, namely, the name *ma-an-sî-na*, found, e. g., in Delaporte and Thureau-Dangin, *Catalogue des cylindres orientaux*

<sup>71</sup> Altogether col. 6, 11-col. 16, 2 contains eighty-one *LAL-eđ-a* forms; col. 6, 11-col. 16, 2 contains thirty-seven, nine of which clearly have *u*, 4, or *e*, and eight clearly *a*, while the remaining twenty cases either do not indicate the vowel in writing or may represent contractions. But we may suppose that at least the forms *gus-da* (col. 6, 15) and *il-da* (col. 6, 18) were indeed as *gu(h)bu-da* and *il-i-da* over against the *gub-ba-da* of col. 15, l. 11, and the *il-la-da* of col. 11, 15. Among the forty-four forms of col. 10, 2-col. 16, 2, one clearly has *e*, while thirty-two have *a*.



I, T 154 and 174: *ma-an-si-na*<sup>12</sup> | *dub-sar* | *dumu-lu-ka-ni*, "ManSina, the scribe, the son of Lukani"; ITT III 6522, obv. 4: *na-ba-ša<sub>3</sub>-dumu-ma-an-si-na-ge*, "Nabaša, son of ManSina"; Reisner, TT, No. 185, obv. 6: [u]r-<sup>4</sup>*ba-ú-dumu-ma-an-si-na*, "Ur-Bau, son of ManSina," etc. But is there any assurance that the root of the verbal form which seems to be contained in the name is *sin* < *sin*?

That certain Sumerian verbal roots appear with both *n* and *m* as their final consonants is a fact well attested; compare, e.g., *giri*n and *giri*m, "fruit," and other words enumerated by me in MAG IV (1929) 167. In almost all of these cases, however, an original *n* has been changed to *m*, while the change from *m* to *n* seems to occur only in rare and isolated cases. Nevertheless, it may be argued that phonetic developments in one direction are frequently paralleled by a corresponding retrogressive development in other cases, and furthermore one might argue that the root *sin*, "to give," which Thureau-Dangin assumes, was perhaps the original form, from which the root *sim* developed by a change of the final *n* to *m*. Obviously, however, speculations of this kind do not furnish any conclusive proof that the combination *sin* in the name *ma-an-si-na* actually represents the root *sin*, "to give." At any rate, in the many hundreds of times the root of the verb for "to give" occurs in such a manner that its last consonant is recognizable, the root is invariably *sin*.

Nor is there any conclusive proof that the sign name *sinu* must have been derived from the root of the verb for "to give," for it would be quite as well possible to assume that the name *sinu* was chosen because the sign was used for some word root *su(n)* or *si(n)* with an *n* as its regular final consonant, and one that had no connection whatever with the root *sin* or *sum*, "to give." As a matter of fact, it would be rather remarkable that a sign should be named after a root form which was not recognized as a good pronunciation of *si*, "to give." The only plausible explanation, in case the name was actually derived from that word, would be that the name *sinu* dates from prehistoric times, and that in that early

<sup>12</sup> In the photographic reproduction of the seal impression on Pl. 11, No. 6 (= T 154) only the first part of the name is visible; judging from Thureau-Dangin's transliteration, however, other impressions of the seal on the tablet furnish all of the missing parts of the name. No photograph of the seal impressions on T 174 is given.

time the root of the verb for "to give" was *sun*, which only later became *sum*; but with such an assumption we would again venture into the field of unprovable conjectures.

No matter, however, what the real origin of the sign name *sunu* was, the fact that the vowel of the name is an *u* would not conclusively prove that the word from which it was derived could have been only *sun*, not also *\*sin* or *\*sen*. This may be illustrated by the name and the phonetic readings of the sign *taĝ*. This sign was known to the Babylonian scribes by the name *šuridu* (written *šu-ri-du*, Syll. c: DT 40 [— CT 11, 31 ff.], col. 4, 33; Syll. c: 38729 — CT 12, 24, col. 1, 54), which would point to a phonetic value *šurid*. As a matter of fact, however, the syllabary AO 7867 (Thureau-Dangin, TC VI, No. 37), col. 2, as already mentioned, gives as phonetic values similar to that name, the values *šerid* (written *še-ri-id*) in l. 33 and *šerim* (written *še-ri-im*) in l. 37, and the much earlier list CT 41, 47, l. 87, finally, gives only the value *šeri* (written *še-ri*). Thus in spite of the *u* in the first syllable of the sign name the syllabaries give us only pronunciations with *e* in that first syllable.

This sign name *šuridu* is instructive in other respects also. For although *šuridu* was the common name of the sign *taĝ* and it might therefore be expected that the value or word *šerid* was very well known, nevertheless the lists give for *šerid* only the scribal note KA-KA-si-ga, which, whatever its exact meaning may be, at any rate indicates that the scribes did not know any definite meanings of this word. Note, moreover, that Syll. c: DT 40 gives as reading of *taĝ* only *taĝ*, although the sign name is given as *šuridu*. It would not be at all surprising, therefore, to find that the phonetic value *sun* or *sin*, on which the sign name *sunu* is based, had no connection whatever with the root *šim*, "to give," but was derived from a word *sun* or *sin* of some unknown meaning. The sign name *sunu*, therefore, is not proof for a reading *sum*. Should some connection between *sun* and *sum* actually exist, however, the case would of course stand just as before; *sun* would then be only a variant pronunciation of *sum* and, like the latter, itself a late pronunciation of the root *si(n)* or *si(m)*. A comparison with the sign name *šuridu* makes this point quite clear, because a phonetic value *sen*, *se(n)* or *sin*, *si(n)* for a sign called *sunu* is, of course, quite as possible as a value *šeri*, *šeri(d)*, *šeri(m)* for a sign called *šuridu*.

#### 4. Summary of Results concerning the Phonetic Values $si(m)$ and $su_{12}(m)$

Summing up the results with regard to the reading of the root  $si$  —  $u\dot{e}d\dot{a}nu$ , we can now assert positively that there is no evidence to prove Thureau-Dangin's assumption that  $sum$  and  $sun$  are the only correct renderings of the root. There still remains the undoubted fact that two Babylonian syllabaries give us  $si(m)$  as the pronunciation of  $si(m)$ , "to give," and that only one syllabary thus far attests to the existence of the pronunciation  $sum$ . Furthermore, all observations point to the fact that the reading  $sum$ , which is given by the Chicago Syllabary, is young, while no such evidence can be adduced for the reading  $si(m)$ , the natural conclusion therefore being that  $si(m)$  is at least comparatively a more original pronunciation than  $sum$ . The problem presented by the two different pronunciations should therefore not be put, as is done by Thureau-Dangin, in such a way that the answer must decide which of the two is correct and which is not. Both are correct, at least when seen in the light of the historical development through which every pronunciation transmitted to us by the various post-Sumerian syllabaries necessarily must have gone during the long periods covered by the lexicographical tradition of the Babylonians. There are, of course, cases where scribes actually made mistakes, but before assuming such a mistake, it would be necessary to prove that no other explanation is possible. As our investigation shows, this is not the case with the reading  $si(m)$ . On the contrary, if it actually were necessary to declare one of the two pronunciations as possibly not genuinely Sumerian, i. e., that it represents a post-Sumerian development, the evidence adduced in this investigation would logically point to the pronunciation  $sum$ . As a matter of fact, however, such an assumption would again be far too hasty, since no one can be absolutely certain that phonetic developments observed in the post-Sumerian period did not have parallels already in the various dialects of Sumerian, even in that form of Sumerian which we call the main dialect. For the more we learn from occasional phonetic spellings in Old Sumerian inscriptions as well as from other observations, the more we become aware of the fact that at the time when it was a living language Sumerian, and especially the pronunciation of its roots, offered a far more complicated aspect than it would seem from the more or

less standardized forms transmitted to us through the post-Sumerian scribal schools. With regard to our problem, this would of course involve the very extensive task of establishing the exact forms of pronunciation of the root *sîm*, "to give," for every locality where Sumerian was spoken, as well as for every period—a task which, however, would require much additional and more dependable material than is available at the present time.

### 5. A General Survey of the Phonetic Values of the Sign *sî*

The immediate purpose of this study having been accomplished, it will nevertheless be quite appropriate to round out our investigations by a more comprehensive study of the historical development of the phonetic values of the sign *sî* and the documentation of this development in the syllabaries. Such a study will give us the necessary background for the development of the root *sîm*.

The Chicago Syllabary passage from which the equation *sî*, "to give" = *sum* is taken gives the following four equations in the section treating of the sign *sî*:

[ se-e	[ sî	[ su-um	ma-šî-lum
[ si-i	[ sî	"	se-pa-um
[ sa-ar	[ sî	"	se-ar-ru
[ su-um	[ sî	"	na-du-um ša-ma

The first two equations <sup>73</sup>

[ se-e	[ sî	ma-šî-lum
[ si-i	[ sî	se-pa-um

give two readings of the sign *sî* which resemble each other very closely and are merely differentiated by the closely related vowels *e* and *i*. The similarity between the two equations extends even farther, since the roots of both *sî* = *mašîlû* and *sî* = *sapânu*, for which the syllabary gives us the pronunciations *se* and *si*, in reality—as we can see from the texts—ended with a *g*, that is, were *seg* and *sig* respectively.<sup>74</sup> On the basis of the equations as we find

<sup>73</sup> We leave out, from now on, the name of the sign.

<sup>74</sup> Cf., on the one hand, *nu-mu-un-da-ab-sî-sî-ga* = *lu maš-lu*, 4 R 9, rev., l. 13 f.; *ma-un-da-ab-sî-gi* = *u-maš-šî-la[-an-sî]*, Reisner, SRH, No. 58, obv. 2; *u-maš-šî-lu*, *ibid.*, rev. 55 f., 83 f. (M 2956); *nig-sî-ga* = *tamšîlû* (*tam-šîl*), B1, 7-27, 130 (=ZA IV, p. 494), l. 6 f. (M 9289); and, on the other hand, *gû-du-a-sî-sî-ki* | *se-šî-lu mât su-i-ri*, King, LHB, No. 50, col. 4, 9; *bi-ib-sî-sî-gi* = *a-sap-pa-ru*, S 954 (Delitzsch, AL, 3<sup>d</sup> ed.,

them in the syllabary, it would seem as though the author wanted to distinguish the two roots as  $seg$  with  $e = mašdum$  and  $sig$  with  $i = sapdum$ . And it must be conceded that it is quite possible that the two really differed from each other by the  $e$ - and the  $i$ -vowel of their respective roots; for in the syllabaries the distinction between  $e$  and  $i$  is, on the whole, well carried through. Nevertheless we cannot be altogether sure that this distinction was really intended by the scribes; for we might have here one of those instances where the original syllabary attributed two variant readings indiscriminately to a sign in each of its meanings, but the later syllabaries, in order to avoid tedious repetitions, listed each of the two readings in only one equation. In other words, the two equations quoted from the Chicago Syllabary may have been abbreviated from the following four original equations:

se-e	st	ma-šd-dum
si-i	st	"
se-e	st	sa-pa-su
si-i	st	"

Be that as it may, it is quite certain that there exists some very close and natural connection between the phonetic values  $se$  and  $si$  of the sign  $st$ ; in other words,  $se$  and  $si$  must be or have been simply deviating pronunciations of one and the same phonetic value or Sumerian word. There are various possibilities of explaining how this differentiation of pronunciation might have come about. It may have been quite original; that is, it may date from the very time when the system of cuneiform writing was adopted by the Sumerians. For their language was spoken in various dialects, and a sign used as ideogram for a certain word would, of course, have been pronounced in the main dialect differently than, for instance, in Emesal. Statements to this effect are still found in the syllabaries, which now and then after having given (or before giving) the usual pronunciation of a sign, that of the main dialect, give a second pronunciation with the remark that it is that of the Emesal. Compare, e. g., Yale Syll. 268 f.

gi-id-ru	ra	" (= gi- <i>es</i> -ru-a)	gi- <i>es</i>
mu-ud-ru	ra	"	" eme-sal

p. 134 ff., rev. 5 (Br. 4420);  $si-si-ga-e-de = ana . . . sa-pa-ni$ , 4 E (2d ed.), No. 3, col. 1, 35 f. (Br. 4420);  $si-ga = na-es-pa-ni$ , Haupt, ASK II, No. 10, rev. 1 f. (M 2100).

and 165 f.

ſ i-za	uuo	" (= u-du-u)	" (= iu-me-rum) eme-sal
ſ u-du	uuu	"	" (= iu-me-rum)

In our case, too, therefore, among other possibilities, sè might very well have been the Emsal pronunciation of a certain word, while si might have represented that of the main dialect.

The other possibility is that the differentiation of the phonetic values under discussion was due to a gradual change of the pronunciation of a certain word written with the sign si. That is, the sign may originally have had perhaps only the value sè because it represented a certain word se; but, after the pronunciation of this word in the course of time changed to si, the sign sè too might have had attributed to it the pronunciation si, though it preserved at the same time its old value sè.

It is, of course, quite impossible with the restricted material at our disposal to decide which of the two possibilities really was responsible for the differentiation of the phonetic values sè and si; it might even be due to both. For our problem, however, this uncertainty is not of decisive importance, since we are mainly interested in getting a clear idea of what sort of connection may possibly have existed between the values sè and si only in order to illustrate their undeniable original identity.

Applying these observations to the root sim, "to give," this root too may be assumed to have been originally sem and only later to have become sim. This assumption is indeed very plausible, since a form with the vowel e is attested by the Emsal form zem, and since there must obviously have been a period with only one single root form of the verb for "to give" from which both the Emsal form and that of the main dialect originated.

If this was really so, it is evident that instead of the first two equations of the Chicago Syllabary a more extensive and more meticulous syllabary would have given the following six equations:

ſ se-e	st	ma-ſd-lum
ſ si-i	st	ma-ſd-lum
ſ se-e	st	se-pa-nu
ſ si-i	st	se-pa-nu
ſ se-e	st	na-da-nu
ſ si-i	st	na-da-nu

or, in another arrangement:

[ se-e ]	si	ma-šá-tum
	si	sa-pa-nu
	si	na-da-nu
[ si-i ]	si	ma-šá-tum
	si	sa-pa-nu
	si	na-da-nu

That the Chicago Syllabary does not give the equations *se-e* and *si-i* = *si* = *nadānu* is, of course, due to the tendency of the syllabaries for utmost economy; it gives for *si* = *nadānu* only the reading *sum*, which at the time when the Syllabary was written and in the school to which the scribe belonged was either the usual or the preferred reading.

This equation *sum* = *si* = *nadānu* is given by the Chicago Syllabary at the end of the section dealing with the sign *si*, a fact which under ordinary circumstances indicates that the equation is a late addition. We have seen already that *sum* can have been adopted as the usual reading of *si* = *nadānu* only in post-Sumerian times; but there is nothing to disprove the assumption that a tendency to change the pronunciation of the root from *sim* to *sum* already existed in the Sumerian period, although perhaps not recognized by the scribes as the regular or the proper pronunciation. In that early period, however, the sign *si* would, of course, have had not the phonetic value *sum* but only the value *su<sub>11</sub>*, since *m* at that time was generally treated as an amissible consonant, at least after *i* and *u* (GSG § 40). In the case of *si* = *šámu*, "garlic," etc., one might even be tempted to regard *su(m)* as a very old reading if there really exists, as is very likely, a connection between the Sumerian word and Akkadian *šámu*, Hebrew *šām*, Arabic *šámun*, and Aramaic *tāmā*. Comparison, however, of Hebrew *šās*, Aramaic *šāsā*, with Akkadian *šišū*, "horses," and of Hebrew *bās*, Aramaic *bāsā*, Minera *bās*, Greek *βασος*, "hyacinth," with Sanskrit *picu*, "cotton," clearly shows that the original Sumerian form of the word for "garlic" could very well have been *si(m)* instead of *su(m)*. A secondary value *su<sub>11</sub>* would also follow from the word *su(n)* < *si(n)* or *se(u)*, from which the sign name *sumu* must have been derived. It is still uncertain, however, at present whether on the strength of the equations *šō* | *sa-pa-nu* (CT 12, 30: 38173, 8) and *šū-šū* = *šápin(uu)* (Br. 7605) an occasional read-



ing *sû(g)* could be assumed even for *sî* (usual reading *si(g)*) — *sapšuu*, because at present we have no means of establishing the fact that the root of *sû* = *sapšuu* really ends with *g*. A scribe who wished to include in his syllabary the old word forms *su(m)* and *su(n)* with their Akkadian equivalents would then have added a third section (cf. p. 73 for the first two sections), which might have read:

† <i>su-u</i>	<i>si</i>	<i>na-du-uu</i>
	<i>sî</i>	<i>šu-uu</i>
	<i>sî</i>	*šuuuu (or some other word)

If this scribe had then wanted to indicate also the post-Sumerian pronunciations of signs with restored amissible consonants, he might have added, either separately after each single section or in collected form at the end of the whole section dealing with *si*, the following equations:

† <i>se-em</i>	<i>sî</i>	<i>na-du-uu, šu-u-uu</i>
† <i>si-im</i>	<i>sî</i>	<i>na-du-uu, šu-u-uu</i>
† <i>su-um</i>	<i>sî</i>	<i>na-du-uu, šu-u-uu</i>
† <i>se-en</i>	<i>sî</i>	.....
† <i>si-in</i>	<i>sî</i>	.....
† <i>su-un</i>	<i>sî</i>	.....

It will be noted that Yale Syllabary 69 gives the Sumerian name of the sign *si*, if Clay's copy is correct, as *zu-û-na*<sup>22</sup> (i. e., *zûna* < *zun-a*). This would indicate a Sumerian value *zun*, or in older times, with omission of the amissible *n*, *zu*, instead of the values *sun* and *su* which we derived from the Akkadian sign name *suuuu* and the Sumerian sign name *su-na* (CT 35, 1 ff., col. 1, 61). Moreover, an unpublished school text from Nippur, containing part of the Nippur Syllabary, gives the following readings:

† <i>zi-i</i>	<i>sî</i>
† <i>zu-um</i>	<i>sî</i> <sup>23</sup>

Finally, it may be recalled that the Emesal word *zem*, "to give," the etymological equivalent of *si(m)*, also shows a *z* instead of the

<sup>22</sup> In the sign name "(= šá-la-gaba-ku)·zu-û-na" (= *i-du*), = *LAGAB* with inserted *si*.

<sup>23</sup> Read therefore also in the duplicate Langdon, UPUM XII 1, No. 55, col. 8 (= Langdon's col. 6), at the beginning of the preserved portion:

† <i>zi-i</i>	<i>sî</i> !
† <i>zu-i-um</i>	<i>sî</i> !

$s$  of the main dialect root forms  $sim$  and  $sum$ . Thus practically all of the phonetic values of the sign  $si$  which we have yet discussed can be duplicated by values beginning with  $s$  instead of  $s$ .










Let us now turn to the equation

$$[za-ar] \quad [si] \quad " (= su-su) \quad [su-ar-su]$$

in Chicago Syll. 123, because the main dialect reading  $zar$  attributed here to the sign  $si$  likewise begins with a  $s$ .

In order to get a firm basis for a correct evaluation of this phonetic value and its connection with the other values of the sign  $si$  it will be necessary for us to investigate the various groups of equations in

a) Yale Syll. 89-91:

[ $za-ar$ ]	 	" -zu- $\dot{a}$ -na- " " "	su-ar-ri $\dot{a}\dot{a}$ $\dot{s}\dot{e}$ -im
[ $.....$ ]	 	" " " " "	su-ra-su
[ $\dot{s}\dot{e}$ -rim] "	  "	" " " " "	gi-gi-tum
[ $\dot{s}\dot{e}$ -rim <sup>88</sup> -sur	  	" $\dot{s}\dot{e}$ -a- " " "	tu- $\dot{a}\dot{a}$ -ru $\dot{a}\dot{a}$ nap-pi-lum

and the corresponding passages of its various duplicates, namely,

B) CT 35, 1 ff., col. 1, 58-61:

[ $za-ar$ ]	 	su-a-su
[ $"$ ]	 	" (= su-a-su) $\dot{a}\dot{a}$ $\dot{s}\dot{e}$ -i
[ $"$ ]	 	su-ra-su
[ $za$ -ri-im] "	  "	gi-gi-tum
[ $\dot{s}\dot{e}$ -dur <sup>89</sup> ]	   	tu- $\dot{a}\dot{a}$ -ru $\dot{a}\dot{a}$ nap-pi-l <sup>90</sup>

<sup>87</sup> I. e.,  $\dot{a}\dot{a}$ -la-gaba-ku-zu- $\dot{a}$ -na-i-du.

<sup>88</sup> See note 88.

<sup>89</sup> See note 89.

<sup>90</sup> I. e.,  $\dot{a}\dot{a}$ -la-gaba-ku- $\dot{s}\dot{e}$ -a-zu- $\dot{a}$ -na-i-du.

<sup>91</sup> Copy or text: za-ri-i $\dot{b}$ . Does the original have  $\dot{a}\dot{a}$ -ri-im?









<sup>92</sup> See note 89.

<sup>93</sup> Note that this reading of the sign deviates considerably from the usual  $\dot{s}\dot{e}$ -rim-sur. Cf. also note 93.

<sup>94</sup> Instead of the signs  $\dot{s}\dot{e}$  and  $\dot{a}$  the scribe inserted the Sumerian sign names(!)  $\dot{s}\dot{e}$ (?) $\dot{a}$  and su-na. King's copy (and the original?) has  $\pi\dot{A}\pi\dot{A}$  (=  $\pi\dot{E}$  +  $\Delta$ ) instead of  $\dot{s}\dot{e}$ -a in both instances.

<sup>95</sup> For this variant compare CT 14, 9; K 4373, Col. 2, 1. 3.

γ) CT 12, 25 f., col. 3, 41-45:




† za-ar			za-ar-rum [                      ]
			za-ar-rum [ša ša'ia]
† su-ar <sup>11</sup>			sa-ra-rum [                      ]
† še-rim <sup>12</sup>			ši-pi-[tum]
† še-rim-sur			še-šā-rū [ša . . . . .]

<sup>11</sup> Note that this text inserts the signs *še* and *š* not only in the fourth, but in all the signs. Brünnow 10234-36 and 10238 f., Meissner 7754-57, and Thureau-Dangin, LHS, p. 30, note 10; p. 31, note 18; p. 32, note 10, all take the sign to be *LAGAB* with inserted *š* instead of *LAGAB* with inserted *še* and *š*; that is, they consider the two inserted signs to be a profane form of *š*. To be sure, the fact that in CT 12, 25 according to Thompson's copy the second inserted sign is added to the first without leaving a space between them, seems to indicate that the scribe actually took the two signs as one. Nevertheless, there can be no doubt whatever that the inserted character represents a combination of *še* and *š*, as is clearly shown by the fact that it appears also as the writing of *še-rim-sur* in the last line of the quoted passage and therefore corresponds to the signs of Yale Syll. 71 and VAT 10172, col. 3, l. 33, which certainly consists of *LAGAB* with inserted *še* and *š* (note especially the description of the inserted signs as *še-a-su-na*, i.e., "*še* and *su*"). Note, moreover, that VAT 10172 gives the same sign instead of the simple *LAGAB* + *š* for *su-a-ra ša še*, a fact which explains the mentioning of *sa-ar-ra* (etc.) and *sa-a-ra* (etc.) *ša še* in two separate lines; for the inserted simple *š* (= *šā*) in the one case corresponds to the simple *sa-a-ra*, while the inserted *še-šā* in the other case corresponds to *sa-a-ra ša še*. The scribe of CT 12, 25 f. then apparently took the two different signs simply as variant forms of the same sign and therefore replaced the simple *zā* in the first, third, and fourth lines by *LAGAB* + *še-š*. As a consequence he even omitted the sign in the second line, since there was no longer any difference between this sign and that of the first line. Similarly, because they too apparently took the two signs as simple variants, the scribes of Yale Syllabary and CT 35, l ff. replaced the *LAGAB* + *še-š* of the second line by the simple *LAGAB* + *š*, and as a consequence the scribe of Yale Syllabary even dropped the entire first line.

<sup>12</sup> Thus according to Thompson's copy. Brünnow 10236 and 10239 reads *zu-ar*.

<sup>13</sup> The reading *še-rim* is proved by Scheil, NVR, p. 5 f., republished by Thureau-Dangin in TC VI, No. 27, col. 4<sub>aa</sub>:

† še-ri-im | raš | šā šā-ran ši-pi-rum

<sup>14</sup> Judging from  = *še-rim-sur*, in which *sur* corresponds to  and *še-rim* to  (= *rim* with inserted *še*), it would seem that the sign for *šerim* alone should be neither *rim* with inserted *še* and *š* (= *sur*), as it is written above, nor *rim* with inserted *š* (= *rim*), as it

3) VAT 10172 (= Assur 3024, not yet published), col. 3, 29-33:

↑ <i>sa-ar</i>	𐎶 𐎶	<i>sa-ar-ru</i>
↑ "	𐎶 𐎶 𐎶 <sup>88</sup>	<i>MI-MIN šá šē sa-ar-ru-m</i>
↑ <i>su-ur</i>	𐎶 𐎶	<i>ša-ra-ru</i> <sup>89</sup>
↑ <i>še-rim</i> <sup>90</sup>	𐎶 𐎶	<i>šē-šē-ru</i>
↑ <i>še-ni-sur</i> <sup>91</sup>	𐎶 𐎶 𐎶	<i>še-še-ru ša [ . . . . . ]</i>

It will be noted that CT 35, 1 ff., ascribes to the noun *zan* = *sa-a-ru* (Yale Syll., *sa-ar-ri*; CT 12, 25 f., *sa-ar-ru-m*; VAT 10172, *sa-a-ru*) as well as to the verb *zar* = *sa-ra-ru* the phonetic value *zar*, whereas VAT 10172 and CT 12, 25 f. give the reading *zar* for the substantive only (written *LAGAB* + inserted *šē* in one case, and *LAGAB* + inserted *šē* and *šē* in the other case), while for the verb *zarāru* (*šarāru*) they give the reading *sur*. However since *zarāru* (*šarāru*) and *zarru* (*sarru*) despite the writings *sa-a-ru* and *sa-a-ru* are evidently of the same root, there is hardly any doubt that *zar* not only as a verb but also as a substantive had, at least at a certain time, the variant reading *sur*, just as the verb was read *zar* and *sur*.<sup>92</sup> A value *sur*, however,

is written in the other texts. Was the correct sign for *šerim* *rim* with inserted *šē*, which later only by mistake was written *rim* + *šē* and *rim* + *šē* + *šē*? Note, however, that this deduction presupposes the correctness of the gloss *še-rim-sur* (or its reading, respectively), which to judge from the variants is not altogether certain. See note 93.

<sup>88</sup> The scribe has inserted into the *LAGAB* sign only the sign *šē*, not the two signs *šē* and *šē* indicated by the sign gloss. Note, however, that he placed the *šē* into the left half of the *LAGAB* sign; evidently he intended to write *šē* into the blank space at the right but forgot to carry out his intention.

<sup>89</sup> Daltzech, *Sum. Glossar*, p. 252, reads *ša-ra-ru*.

<sup>90</sup> See note 86.

<sup>91</sup> See note 89.

<sup>92</sup> Whether the readings *še-ni-sur*, *še-rim-sur*, and *še-dur* can be phonetically harmonized is still quite uncertain. It is not very likely, since the syllabary glosses use only the most common phonetic values, which undoubtedly are *nī* and *zā* for *ni*, and *šir*, *šil*, *šub*, and *rim* for *LAGAB*. Possibly the three variants represent synonymous expressions, as, for instance, *še-dur*, "the *dur* of *še*," *še-ni-sur*, "the *sur* of *še-ni*," and *še-rim-sur*, "the *sur* of *še-rim*." On the other hand, it is quite possible that the differences are due to mistakes of the scribes who copied the texts.

<sup>93</sup> Similarly also *gal*, "great," at some time must have been pronounced *gul*, which later appears only as root of the verb *gul*, "to be (or to make) great" (so, e. g., also in the verbal adjective *gul-a*, written *gu-la*, *gū-la*, "great").

must have been attributed also to the simple *sî* — *zâr* — *sarru*. For, since the sign *LAGAB* + *sî* — *zar* contains the sign *sî*, which by itself already expresses the syllable *zar*, it is evident that the exponent of the readings *zar* and *sur*, for *LAGAB* + *sî* was the sign *sî*. Note furthermore that in *šerimsur* also the element *sur* corresponds to the *sî* of the compound sign.

With the value *sur*, *zur* for *sî* we gain an immediate contact with the phonetic values *zu(m)*, *su(m)*, and *zu(n)*, *su(n)*, and indirectly also with the values *ze*, *se* (= *ze(g)*, *ze(m)*, *ze(n)*, etc.), and *zi*, *sî* (= *si(g)*, *si(m)*, *si(n)*). For just as the sign *sî* with the value *zu* or *su* was used to render in writing the first part of the root *zum*, *sum*, "to give," just so it could, of course, be used to express the first part of the root *zur*, *sur*.

This at once brings up the question whether the value *zar* which is attested for *sî* by the syllabaries, or the value *zur*, which we obtained by a logical deduction, is more original. The value *zar* is placed by all syllabaries at the head of the section that treats of the sign *ZAR*. This fact would certainly be significant if it could be established that the syllabarist gives the values in the order of their historical origin. In *Chicago Syllabary* and in *CT 35, 1 ff.*, *zar* is even given as the only reading, at least as far as the equivalents for *zâru* and *zarâru* are concerned. Moreover, the loan word (?) *sarru* or *sâru* in Akkadian also presupposes a Sumerian prototype *zar*, not *zur*. *Sur*, on the other hand, appears only as variant of *zar* = *zararum* in *CT 12, 25 f.*, and the Assur text, and as phonetic reading of *sî* in the compound *šerimsur*. The conclusion, therefore, seems to be justified that *zur* was a secondary pronunciation of later date than *zar*, just as the pronunciation and root form *sum* instead of *si(m)* or *se(m)* is of a comparatively late date. However, despite the fact that the value *zar* must be regarded as the more original value, it too in its form *zar*, with the final *r* pronounced, undoubtedly belongs to the post-Sumerian period. The corresponding pronunciation of the Sumerian period, which dropped the final *r*, must of course have been *za* only.

But then we are confronted by the question concerning the relation of this value to the values *ze* or *se*, which, as shown above, were likewise older than the values *zu*, *su*. The most reasonable answer seems to be that *za(r)* is secondary and goes back to a more original pronunciation *ze(r)* with an open *e* as its vowel. This *e*

may first have become a sound like the *ä* in German "Fülle," or the *a* in English "fat," and only later on become a more rounded *u*, as, e. g., in German "Wasser." Utilizing the observations made concerning the vowels of certain Sumerian verbal roots in our study on the prefix forms *e-* and *i-*, we should assume that the *u* of *zur*, *sur* (= *zarurw*), which had developed from *zar*, was, at least originally, also an open vowel, that is, an *o*, the pronunciation of the root then being in reality *zor* instead of *zur*.<sup>90</sup>

<sup>90</sup> For quite the same reason the root *gal*, which parallels the root *gail*, "great," must have been pronounced *gol* with an open *o* corresponding to the open vowel *a* of *gal*. As further cases of a pronunciation of *u*, which originated from *e*, as *o*, may be mentioned the nouns *geštar(u)* from *geštar(u)*, and *mišša*, "weapon," from *mišša*. It will be noted that the Akkadian scribes in the case of *geštar(u)* [written *geš-tur*... Chicago Syll. 229; *giš-tu-ru*, Yale Syll. 253], and *mišša* (written *mi-iš-tu*, CT 25, 1 ff., col. 2, 30) render the syllable *ša* with the Akkadian sign *tu*, the emphatic *š* of which will naturally cause the following *u* to sound more or less like *o*. Also the participle *bad-du* (< *bad-a*) must have been pronounced *baššo*; note the writing of the phonetic value of *naš* as *baš-tu* in Syll. a (CT 11, 1 ff.), col. 5, 32. For the existence of an open *o* in late Akkadian, compare, e. g., the rendering of *Nabû-budur-ušur* with *naḫḫa-šur-ušur* (LXX) and *naḫḫašur-ušur* (Lucian) in Greek, and 𐎠𐎢𐎩𐎠𐎫 (and sim.) in the Massorah. For transition of *u* into *o* in late times cf. Greek *rupos* (and Mass. 𐎠𐎢𐎩𐎠𐎫) from Akk. *Rûraš*. For the pronunciation of *u/u* as *o* in Sumerian in early as well as late times, cf. *domu*, "child," and its secondary form *damu* (gloss *du-mu* in 𐎠𐎢𐎩𐎠𐎫=PUMU; see AJSL LI [1934/35], p. 109); the frequent dialectal forms (written *tu-mu*, e. g., 4 R. 27, No. 1, 6); the rendering of *dumu* with *šau* (with the usual dropping of a final short vowel in late times) in *gišgišimmar-domu-domu* | *ta-š-šau* = 𐎠𐎢𐎩𐎠𐎫 *šau* (< *šau*) *šau*, PSBA, 1902, p. 100, 1; Hebr. 𐤔𐤌𐤕, the god and month "Tammûz," from Sum. *da(m)mu-zi* (< *damu-zi(d)*) or *ta(m)mu-zi*; Syr. ܬܡܘܙܐ, (sometimes also ܬܡܘܙܐ = *te(m)muš*), "month of Tammûz," from Sum. *tamu-zi*. The *t* of Hebrew and Syriac points to a loan directly from Sumerian at a very early time. (For such borrowings directly from Sumerian see Poschel, "The Name of Elam," in AJSL 48, [1931/32], 20 ff.) For old Sumerian *i* instead of late Sumerian *ä*, note, e. g., the use of *PUMU* (= *tumu*) as equivalent of *tu-mu* in the old text AO 4331 + 4335 (Thureau-Dangin, NPT, p. 206) see my transliteration and translation in Sumerische Untersuchungen II, pp. 12 ff. (= ZA, N. F., III, p. 38 ff.), col. 3, 6 (Zimern's assumption in *Tamuz*, p. 703, of a secondary Semito-Babylonian pronunciation *tamšau* lacks proof); further, the use of *du*, *du*, etc. for *tu* and *tu* in the older Akkadian system of writing, a use that must, of course, go back to the Sumerian system, which the Akkadians merely adapted to the needs of their language. The doubling of the *m* in Hebrew and the length of the first vowel in Syriac,





# HEBREW "HELMET," A LOANWORD, AND ITS BEARING ON INDÓ-EUROPEAN PHONOLOGY

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AMONG THE Biblical Hebrew words which have long been suspected of being loanwords rather than derivable from Semitic roots is the word for "helmet," *kôḥā'*, construct state *kôḥa'*, plur. *kôḥā'im*; a parallel form is *qôḥā'*, construct state *qôḥa'*. The occurrences are:

- 1 Sam. 17: 5: *w'kôḥa' n'hôḥēl 'al-rôšd* "and he [Goliath, the Philistine] had a helmet of copper upon his head "
- 1 Sam. 17: 38: *w'nôḥēn qôḥa' n'hôḥēl 'al-rôšd* "and he [Saul] put a helmet of copper upon his head " [var. lect. *kôḥa'*]
- Is. 59: 17: *w'kôḥa' y'šā' b'rôšd* "and (he put on) a helmet of salvation upon his head "
- Jer. 46: 4: *w'hiṭyass'hā b'kôḥā'im* "and stand forth with (your) helmets "
- Ezek. 23: 24: *šimū mōḡēn w'qôḥā' yôšimū 'alāyik sâḇiḥ* "buckler and shield and helmet shall they set up against thee round about "
- Ezek. 27: 10: *mōḡēn w'kôḥā' tilla-ḥāḥ* "shield and helmet they hung up in thee "
- Ezek. 38: 5: *kullām māḡēn w'kôḥā'* "all of them [Persia, Ethiopia, and Libya] with shield and helmet "
- 2 Chron. 26: 14: *wayyôḥēn lāḥēm 'uzziyyāḥā l'ḥol-ḥazzāḥā mōḡinām urmāḥām w'kôḥā'im w'siryānōḥi uqāḥōḥi* "and Uzziah prepared for them, for all the host, shields and spears and helmets and coats of mail and bows "

The word *kôḥā'* lingers on in post-biblical Hebrew with the meanings of "helmet" and "turban," also "thyroid cartilage, Adam's apple" (obviously a metaphorical transfer). It was doubtless in popular use in the Aramaic dialects, as we may judge from the occurrences of *kôḥā'* and *qôḥā'* in the Jewish Aramaic Targums, again in the two senses of "helmet" and "turban," more particularly "priest's turban," and from the Syriac *qôḥā'* (or *qubbā'*?)

<sup>1</sup> According to Nöldeke. See Ges.-Fuhl, sub *qôḥā'*.

"cowl, cape, hood" and, again in a transferred sense, "capital of a column." (The Syriac *kāḥā'd* "helmet," on the other hand, is perhaps merely a bookish copy of the Biblical Hebrew *kāḥā'.*) Jewish Aramaic *kāḥā'd*, *qāḥā'd* and Syriac *qāḥā'd* (*qabbā'd*) do not agree on the basis of a common Semitic form and meaning (say \**kauḥa'-u* or \**qauḥa'u* "helmet") but on the basis of a secondary borrowing of a West Aramaic *qāḥā'*, with suffixed article *qāḥā'd*, "turban" (m.; cf. plural *qāḥā'īn*, with article *qāḥā'ayyā*) as *qāḥā'd* (*qabbā'd*). The older meaning of "helmet" was no longer strictly applicable in folk Aramaic, one may guess, and the Palestinian Aramaic -ā- was replaced in the borrowing Syriac by its nearest phoneme -a- (with variant -u- followed by doubled consonant). Had the Syriac form been a true development of the \**qauḥa'u* which lies back of Hebrew *qāḥā'*, we should have expected a Syriac \**qauḥā'd* (cf. Syriac *mautā* "death": Jewish Aramaic *māḥā'd*: Hebrew *māḥ* < Sem. \**maui-u*). It is on the basis of a specifically Syriac *qāḥā'd* > *qabbā'd* that the Arabic culture loanwords *qub'-a*, f. *qubba'-at-u*, "hood" are most easily understood; Ethiopic *qabbā'* too, with its -ā- (or zero) instead of original Sem. -a- is not directly traceable to \**qauḥa'u* but to some Aramaic prototype. The word seems to have no Accadian cognate. The actual Semitic occurrences are, therefore, limited, in effect, to Hebrew and Western Aramaic, with subsequent borrowings in Eastern Aramaic (Syriac), Arabic, and Ethiopic, and do not suggest a common Semitic word, but a specifically Hebrew word. This lingered on, mostly with the transferred meaning of "turban" rather than "helmet," in the Palestinian Aramaic dialects which superseded Hebrew and had borrowed the word from it. And, in turn, a lone Hebrew word for an object of such distinctive cultural connotations as "helmet" is likely to be of foreign origin.

The most likely guess would be that the word was borrowed from a language possessing a voiceless *k* that was unaspirated and was not strictly identifiable with either the normal Semitic *k* (ordinarily aspirated when non-emphatic: *k'*) or its emphatic correspondent (ordinarily velar and unaspirated when emphatic: *k* = *q*). Such a *k* would have to belong to a non-Semitic language; e. g., the Greek *κ* or, presumably, the Hittite fortis *k* (*k*:-*-kk*-) would be a case in point. Obviously, an original \**kauḥa'-*, with unaspirated *k*, agrees with Semitic *k* (= *k'*) in position but with Semitic *q* in its lack of aspiration. It is well known that at a much later period a

host of Greek words with *κ* were borrowed by Jewish Aramaic and Syriac with change of *κ* to *q*. The Hebrew variants *kōbā'* and *qōbā'* are suggestive internal evidence for the non-Semitic origin of the word and it was precisely this alternation that led Barth<sup>2</sup> to speak of the word as of doubtful origin. The formative type to which it would have to be referred as a native Semitic word (*qōtal*) is very sparsely represented in Hebrew and the radical from which it might be derived (*kō'* or *qō'*) either non-existent in Hebrew or clearly irrelevant (*qābā'* "to rob one of something"). A variant *kō'* of the common radical *qb'* "hill" is most unlikely.

Philistine, a non-Semitic language contiguous to Hebrew on the west but originally spoken in Asia Minor, is suggested by the first of our biblical citations as a possible source.<sup>3</sup> If this is correct, it would indicate the presence in Philistine of a voiced laryngeal or, more likely, velar spirant: *γ* (— Arabic *ḡ*ain) rather than *ʿ* (Arabic *ʿ*ain). The prototype of the Hebrew word would be something like *\*kaubay-*. The cultural evidence points to the helmet, in various forms, as originally more properly at home in Asia Minor than in Palestine.<sup>4</sup> We are, therefore, not unprepared for Hittite *\*kūpaḫi* (acc. *kūpaḫia*, pl. *kupaḫais*) "hat, cap?"<sup>5</sup> The *-ai-* or *-a-* of Hittite *kūpaḫi-* may go back to an IE *au* (cf. Hitt. *ḫuḫḫas* "grandfather," Latin *avus* < IE *\*auros*),<sup>6</sup> while the single *-p-* points to a lenis stop,<sup>7</sup> which would be heard by the Semitic ear as *-b-*. The word may have come to Hebrew directly from Philistine or it may have gradually worked its way south from some dialect (with *-au-* for Hitt. *-ā-*) of the Hittite-Luwian group through Syria, in which case the Western Aramaic forms quoted above represent a culture borrowing from the north rather than from Hebrew.

The most interesting phonologic point raised by the comparison of Hittite *kūpaḫi-* with pre-Hebrew *\*kauba-* (*\*kaubay-*) is the correspondence of Hittite *-ḫ-*, written single, with Hebrew *-b-*. As is well known, Hebrew and Aramaic *b* represents a pool of Semitic *b*

<sup>2</sup> *Die Nominalbildung in den semitischen Sprachen* § 38a2, note 2.

<sup>3</sup> See, e.g., A. S. Macalister, *The Philistines* 80.

<sup>4</sup> See, e.g., Kurt Gallug, *Biblisches Reallexikon*, sub "Helm"; M. Lidzbarski, *Epikomeris für semitische Epigraphik* II. 135 (Shardana warriors); P. Thomsen in Ebert, *Reallexikon der Vorgeschichte* V. 287.

<sup>5</sup> E. H. Sturtevant, *A Hittite Glossary*, 2nd ed., 83.

<sup>6</sup> E. H. Sturtevant, *A Comparative Grammar of the Hittite Language*, 101.

<sup>7</sup> *Ibid.*, 73-83.

(voiced laryngeal spirant) and  $\gamma$  (voiced velar spirant), kept apart in Arabic as  $\text{ʾ}$  and  $\text{ğ}$  respectively. The primary value of Hittite  $\text{ḫ}$  was probably that of a voiceless velar spirant ( $x$ ), as in Accadian, and this value would normally be rendered in a Hebrew loanword as  $\text{ḫ}$ , e. g. *ḫitti* "Hittite": Hit. *ḫatti*. I have pointed out elsewhere, however, that in certain cases Hittite  $\text{ḫ}$  must, in all probability, be interpreted as the corresponding voiced (or lenis) velar spirant  $\gamma$  [this rather than  $\text{ʾ}$ ], e. g. *ḫapalis* "vessel" [read *γapalis*]: Canaanite *\*abadima* "slaves" (Hit.  $\gamma$  as nearest native phoneme to represent Semitic  $\text{ʾ}$ );<sup>9</sup> *Tuthaliyas*: Hebrew *Tiḏāl*<sup>10</sup> (Hit.  $\gamma$  borrowed as  $\gamma > \text{Can. } \text{ʾ}$ ).

In the example before us a Hittite  $\text{ḫ}$  is found in intervocalic position. On general principles we should expect such a  $\text{-ḫ-}$  to have the value of a voiced spirant,  $\text{-}\gamma\text{-}$ , in contrast to the voiceless value of  $\text{-ḫḫ-}$  (e. g. *paḫḫur* "fire" — *paḫur*), for, as Sturtevant has shown, doubled stops in intervocalic position are voiceless (probably voiceless fortis), while single stops are voiced (more probably voiceless lenis). In other words,  $\text{-p-}$  (written  $\text{-pp-}$ ):  $\text{-s-}$  (written  $\text{-p-}$ ) as  $\text{-c-}$  or  $\text{-xc-}$  (written  $\text{-ḫḫ-}$ ):  $\text{-}\gamma\text{-}$  or lenis  $\text{-x-}$  (written  $\text{-ḫ-}$ ). Very likely  $\text{-ss-}$ : the less common  $\text{-x-}$  represents a similar contrast between  $\text{-c-}$  (fortis voiceless affricate  $\text{-ts-}$ ) and  $\text{-}\gamma\text{-}$  (lenis voiceless affricate  $\text{-vx-}$ ). The pair  $\text{-ss-}$ :  $\text{-s-}$  too may mean something more than a purely orthographic variation. The problem of the meaning of Hittite single and doubled consonants would seem to deserve further study. If we are right in reading Hit. *kūpaḫi-* as *kūsayi-* < *\*kousay-i-*, we are not far from discovering an actual example of the posited IE  $\gamma$  (Kurylowicz's  $\text{ṛ}$ ) in a recorded Indo-European word of an Indo-European language, for it would be difficult to disconnect pre-Hittite *\*kousay-i-* (with derivative  $\text{-i-}$ :  $\text{-ya-}$  suffix?<sup>11</sup>  $\text{-a-}$  softened from IE  $\text{-p-}$  after  $w$ ?<sup>12</sup>) from the large group of IE words represented by Germ. *\*χauðu-ð-a-* "head" and Latin *caput* (dissimilated from *\*kaupa-*?). The intricate phonological problems that are further suggested by this (e. g., Latin *cappa* < *\*kapy-ā* < *\*kaupy-* because of analogy of *caput*?) are not our present concern.

<sup>9</sup> A, n, c are here used for lenis voiceless stops.

<sup>10</sup> See *Language* 10, 274-79.

<sup>11</sup> *Ibid.*, 276-77.

<sup>12</sup> Cf. Hittite *keeris* "glove": *keesar* "hand."

<sup>13</sup> Cf. *JAOI* 58, 291.

Sturtevant's interpretation of Hittite *-eḫ-* before vowels (*seḫur* "urine"; *meḫur* "time"; *weḫ-* "to turn") as an orthographic method of representing an IE prevocalic *\*-e-*<sup>12</sup> is not consistent with our theory, for it is difficult to believe that Hit. single *-ḫ-* between vowels could mean both *-y-* and *-i-*. In another connection some evidence, of a different type from that here presented, will be given that makes it possible to interpret Hit. *-eḫ-* not as *-e-* but as *-ey-*. In other words, it will be suggested that while IE. *\*-ex-* regularly remains in Hittite as *-aḫḫ-*, IE *\*-ey-* (at least before vowels) similarly remains as *-eḫ-*; *-e-* of theoretical IE *\*-ey-* does not undergo a change to pre-Hittite *\*-ey-*<sup>13</sup> (in most IE dialects pre-consonantal *\*-ey-* becomes *\*-ey-* heavy-base *\*-ō-*).



<sup>12</sup> *Language* 12, 180-87.

<sup>13</sup> A number of other cases have been noted of Hittite *-ḫ-*: Canaanite *-i-*. These will be discussed at another time.

## THE JAPANESE SHŌEN (荘園), OR MANOR: SOME USEFUL TERMINOLOGY<sup>1</sup>

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THE TERM *shōen* is composed of two parts. *Shō* means a *bessō* (country home), while *en* means well-kept grounds. These grounds may be parks or cultivated farm lands. In other words, then, a *shōen* was a country estate or manor.

A typical *shōen* consisted of a core of rice fields, surrounded by dry fields, in a section of which were clustered the farmers' huts, with a fringe of waste land forming the outer border and stretching away to the next agricultural settlement. The *shōen* gorged themselves on waste land, turning large tracts into *konden* (new rice fields), and into dry fields, and then often divided like amoeba. Where civilization was highest and the population the thickest, *shōen* generally tended to break down into small units composed primarily of intensively cultivated rice fields. Where the population was sparse and backward, and where agricultural methods were comparatively primitive, however, the *shōen* were quite large, contained a great deal of waste land, and produced rather little per acre of rice and dry fields.

*Shōen* originated in several ways. A few came from *tsudokoro* (private estates) of clan aristocrats that had never been incor-

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<sup>1</sup> Most of the material for this paper was obtained from the following three sources: (1) *The Development of the Manorial System (Shōen sei no hattaten)*, by Nishioka Toranosuke; published by the Iwanami Shoten, Tokyo, 1933, in its series of *Iwanami Lectures on Japanese History (Iwanami hōen, Nihon rekishi)*; (2) *The Organization of the Manors in the Heian Period (Heian-cho jidai ni okeru shōen no seishiki)*, by Kawakami Tasuke; in Nos. 3 and 4, Volume XI of the *Journal of History (Shioki)*, July and October numbers, 1926; published by The Historical Society (Shigaku kenkyū kai), The Kyōto Imperial University; (3) *The Manorial Policy of the Heian Court (Heian-cho no shōen seisaku)*, by Kawakami Tasuke; in Nos. 2, 3, 6, and 7, Volume XXXIV, February, March, June, and July numbers, 1923; in *The Historical Magazine (Shigaku zasshi)*, published by The Historical Society (Shigak-kai), The Tōkyō Imperial University. This study is confined to the period 845-1105 A.D. No attempt has been made to treat the *shōen* in the Kamakura and Ashikaga periods.

pointed in the *handen* (allotted rice fields) system. Many were composed wholly or in part of *jiden* (temple rice fields) or of *shinden* (shrine rice fields), as these were *fuyusoden* (tax free rice fields) that had been granted by the government in perpetuity to the great Buddhist temples and Shintō shrines. Some grew up from *kōden* (merit rice fields), for although these were *yusoden* (taxable rice fields), *daikōden* (great merit rice fields) were granted in perpetuity, *jōkōden* (superior merit rice fields) could be inherited by the receiver's sons and grandsons, and *gekōden* (inferior merit rice fields) passed on to the original receiver's sons. *Daikōden*, therefore, were really private property from the time they were granted, while *jōkōden*, and to a less extent *gekōden*, often became so because they were frequently never returned to the government. Then there were *shiden* (gift rice fields) presented to men for some special work they had done. These were held for life and were sometimes handed on to descendants, thereby easily becoming private lands. *Iden* (rank rice fields) were granted to officials of the *Go-i* (Fifth Rank) and above for as long as they held such ranks; but as these officials were seldom demoted or deprived of their ranks, they held each *iden* for the rest of their lives, and the lands were frequently transferred to sons of high rank on the deaths of the fathers. Consequently, many *shiden* and *iden* soon became private property in all but name, and found their way into the *shōen*. The same thing held true for the *shikubanden* (office rice fields), only it was even easier for these to be incorporated in *shōen*, as they were *fuyusoden*. There were several other kinds of tax free rice fields that were easily changed into *shōen*, such as the *ekiden* (biennial rice fields), termed such because they were so poor that they grew a crop only every other year, the *unemedan* (Palace Women's rice fields) held by the *Uneme* (Palace Women) who served in the *Kōkyū* (Women's Quarters of the Imperial Palace), the *shikiden* (post rice fields) given to those who served on the empire's post roads, the *shaden* (archery rice fields) bestowed on those who had shown proficiency with the bow, and so forth. All these types of rice fields, either because they were held for long periods of time, or because they were tax free, turned rather easily into *shōen* whenever the government relaxed its hold on them. Most *shōen*, however, were composed of ordinary *kubunden* (sustenance rice fields) that had been commended to the owner of the *shōen* by the farmers who



had been allotted these lands under the *konden* system, and of *konden* that had been reclaimed from waste land by wealthy land-owners who had the necessary capital to invest in such undertakings. The government lost its hold on the *kubunden* when it stopped taking a census every six years and re-allotting lands at that time, while it ceased to control waste lands when it permitted private individuals to reclaim unlimited amounts and to till these *konden* without paying taxes on them for long periods of time.

The farmers that tilled the fields of the *shōen* were as different in their origins as were the lands they cultivated. Some were descendants of *nuki* (slaves) that had belonged to the clan ancestors of the lord of the *shōen*, while some were runaway *nuki* from other estates. Some were farmers who had fled from the imperial lands and abandoned their *kubunden* because of excessive taxation, some had been forced to seek employment in the *shōen* because they had had the misfortune to be born in over populated sections of the country where the available agricultural land was so limited that the government had been unable to grant them their rightful amount of *kubunden*, and some were wanderers by nature who drifted from one *shōen* to another. Many were members of *jūkiŕu* (sustenance households) that had been granted to men of high rank as *ifu* (rank households), *shōkiŕu* (office households), or *kōfu* (merit households).

The ultimate object in establishing a *shōen* was to withdraw its lands from the public domain and have them declared private property, get its farmers struck off the government lists and recognized as people belonging to the manor only, win exemption from the *so* (tax of rice on paddy fields) for all rice lands in the *shōen*, and exemption for all males living in the *shōen* from the payment of *chō* (tribute in kind), *yō* (commuted tax in place of forced labor), from rendering *edachi* (regular annual forced labor), and *yōeki* (additional forced labor), from having to accept *saike* (loans) from *Kokushi* (Provincial Officials), and from being conscripted for military service, and finally to win the right to prevent all government officials from entering the *shōen* for any reason whatsoever. When recognition of all or at least most of these rights had been won from the *Kokushi*, the *shōen* was called a *kokumenshō* (provincially exempted manor), and when these rights had finally been recognized by the *Dajōkan* (Great Council

of State), the *shōen* was known as a *chōkumenshō* (imperiallly exempted manor). A *chōkumenshō* that had won all the above mentioned rights or privileges was, therefore, a completely independent political and economic unit. It was a state within the state.

The waste land of a *shōen* was used in common by all the *shōmin* (people of the manor), but the rice and dry fields were held by individuals. The common farmers were known as *gorindo* (villeins). There were two types of *gorindo*, those who tilled *kubunden* they had entrusted to the *Ryōshū* (Lord of Manors) and hence were owners of these fields, and those who had come as landless laborers to the *shōen* and had been given *konden* to till, and therefore were renters. *Shōen* frequently contained estates owned by *Jinushi* (Land Masters), who were sometimes called *Tato* (Rice Field Masters). These *Jinushi* or *Tato* were landowners who were not powerful enough to become *Ryōshū* themselves, but entrusted their lands to *Ryōshū* and then served as officials of the *shōen*. In almost all cases they had *gorindo* tilling most of the soil for them, although some of the fields they held directly, these being known as *tsukuda* (directly held rice fields). On these *tsukuda* the *Jinushi* generally paid nothing in the way of taxes to the *Ryōshū*, the income from such lands being considered as his salary as an official of the *shōen*. *Shōen* that were composed solely of *konden* had no such *Jinushi*, and officials in that type of manor held *kyūden* (tax free salary rice fields) instead of *tsukuda*.

The central figure in the whole *shōen* system was the *Ryōshū*, or Lord of Manors. His rights in the land were threefold, namely, those of disposal, administration, and taxation. He was entitled to several forms of revenue from his *shōen*. These consisted of the whole harvest from his *tsukuda*, *jishi* (land rent) from *konden* tilled by *gorindo* as renters, *nengu* (annual tribute) from fields cultivated by *gorindo* as petty landowners, *nengu* from all the fields of *Jinushi* except their *tsukuda*, and forced labor from all the *shōmin*. In addition, he could levy special payments and demand special services on occasion. The more warlike of the farmers and *Shōshi* (Manorial Officials), moreover, were required to render police and military service.

The administrative officials required to govern these *shōen* were generally known as *Jōshi* (Upper), *Chōshi* (Middle), and *Geshi* (Lower Family Officials). Starting at the bottom of the scale,

the *Geshi*, who were also called the *Shōshi*, or Manorial Officials, consisted of the *Jinushi* or *Tato*, those officials who held *kyūden*, and the more capable and warlike *goriade* among the ordinary *shōnin*. These officials represented the interests of the people of the manor and were entrusted with the ordinary administration of the *shōen*. In time, as the *Ryōshū* came to own many *shōen* scattered all over the country, it was found necessary to send *Chūshi* to the *shōen* to look out for his interests and to see to it that the *Geshi* or *Shōshi* were sending him the right amount of taxes and carrying on the work of the *shōen* properly. These *Chūshi* generally set up *Asukaridokoro* (Manorial Managing Offices), within the more important *shōen* and took over the general supervision of the manors. They usually received taxes from certain lands within the *shōen* as their salaries.

At the top of the administrative scale were the *Jōshi*. They met at the main residence or headquarters of the *Ryōshū*, and if he was a *Kugyō* (High Court Noble) and the head of a great family, these *Jōshi* were called *Kashi* or *Keishi* (Family Officials) and formed a large part of the personnel of the *Mandokoro* (Supreme Administrative Council), the *Hyōjōshū* (Supreme Advisory Committee), the *Monchūjo* (Supreme Court), and the *Samuraidokoro* (Supreme Military Council), which had charge of conducting the *Ryōshū's* multitudinous and complicated affairs as head of a great family and owner of many *shōen* as well.

In cases where a great temple or shrine was the legal *Ryōshū*, statues or relics of the *Butsu* (Buddhas) or *Kami* (Deities) worshipped at these places were often set up in the *Asukaridokoro* of the *shōen*. These offices then became *matsujō* (subsidiary temples), or *massha* (subsidiary shrines).

Above the *Ryōshū* was the *Honke* (Legal Guardian of Manors). This post was held either by a *Kugyō*, or a member of the Imperial Family, an influential lady in the *Kōkyō*, or by a powerful temple or shrine with strong court connections. It was the duty of the *Honke* to persuade the *Dajōkan* to proclaim the *shōen* a *chokumenshō*, and to threaten the *Kokusshi* with dismissal from office and punishments if they dared invade the *shōen* under this guardian's protection. In return, the *Ryōshū* sent the *Honke* a fixed yearly income previously agreed upon. If the *Honke* was successful in obtaining further rights or privileges for the *shōen*, his income was

increased; but if he failed to guard the *shōen* from adverse legislation and the attacks of the *Kakushi*, the *Ryōshu* generally broke his connections with such an unsatisfactory protector and found a more efficient *Honke* for his estates. Sometimes members of the Imperial Family, or *Sesshō* (Regents) and *Kampaku* (Civil Dictators) of the great Fujiwara family would grant themselves large tracts of the public domain, and then after reserving for themselves the post of *Honke*, distribute these lands among *Ryōshu* so as to free themselves from the burden of administering their estates. In such cases, of course, the *Ryōshu* was acting illegally if he broke his connections with the *Honke* and placed the lands under the protection of another. It can be seen at a glance, however, that the key position in the whole *shōen* system was not held by the *Honke*, but by the *Ryōshu*.

Despite the fact that many disputes were settled by appeals to force, and that the tendency to do so steadily increased, the *Heian* Era was one in which a written land document carried a remarkable amount of weight. The *gorindo*, *Jinnushi* or *Tato*, *Geshi* or *Shōshi*, *Chūshi*, *Jōshi*, *Ryōshu*, and *Honke* all possessed documents guaranteeing them rights of office and rights in the land. These were generally perpetual rights, inheritable by women as well as by men, transferable with the permission of the *Ryōshu*, and capable of being divided and subdivided almost indefinitely. They practically constituted a form of paper currency. Hundreds have been preserved to the present day, and it is from these that a detailed description of the *shōen* system can be made.

## STUDIES IN ORIENTAL ARCHAEOLOGY

### I. PLANO-CONVEX BRICKS

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DELOUGAZ has recently summarized our knowledge about plano-convex bricks and has advanced an hypothesis concerning their origin.<sup>1</sup> If I understand him correctly, he even offers two hypotheses which, however, are mutually exclusive. He says, p. 25: "Their peculiar form did not come about through any architectural or technical need, but merely as a result of rather primitive craftsmanship"; and, p. 37: "the peculiar form resulted from the brickmaking technique." He has, indeed, shown beyond doubt that the rectangular lower part got its shape from the wooden frame into which the lump of clay was laid and that the upper bulge is the surplus of the rolled lump not taken off. But why was this surplus taken off in the preceding period and left in the "plano-convex" period? The answer is obvious: the brickmaker wanted to retain this shape. Not the "technique" or "craftsmanship" is responsible for it, but the peculiar ideas of the architect who made some special use of the shape. I therefore think that Mr. Delougaz' second hypothesis is preferable, namely, that the plano-convex form imitates stone flakes for which such a shape is natural. This is the opinion also of other archaeologists, especially of Dr. Speiser.<sup>2</sup> Dr. Speiser argues clearly and irrefutably as follows: Since plano-convex bricks replaced the much more practical former type of wholly rectangular bricks, and since the queer plano-convex shape

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<sup>1</sup> P. Delougaz. 1. Plano-convex Bricks and the Methods of their Employment. 2. The Treatment of Clay Tablets in the Field. The Oriental Institute of the University of Chicago. *Studies in Ancient Oriental Civilization*, Nr. 7, 1933.

<sup>2</sup> *AJA* XXXVII, 1933, 461. Also Jordan, 2 n. 3 *Berichte über die Ausgrabungen in Uruk-Warka* = *Abh. Preuss. Akad. Phil.-Hist. Kl.*, 1931, Nr. 4, 18; 1932, Nr. 2, 37, and E. Heinrich (*Schilf und Lehm*, Berlin, 1934, 52 f.) hold "that the plano-convex bricks were introduced by newcomers from a country in which stone was abundant." Doubts are expressed by Woolley (*Ur Excavations* II, 229 ff.) and Mallowan and Rose, *Iraq*, 2, 1936, 16.

can be understood only as an imitation of stones of such form, the people who first used the plano-convex bricks were conquerors coming from a country outside of Mesopotamia.

Delougaz supports his theory that the plano-convex bricks imitate stone by other arguments.<sup>6</sup> As the shape, so also the arrangement in "herringbone" pattern is natural for stones of this type and is actually found in various places in our times, namely around Mosul, in the Anti-Lebanon and in Southern France. There are also other features of architecture during the plano-convex period which point to an origin in a hilly country, such as columns, round fortifications, and others. Delougaz' ideas are perfectly sound and can be corroborated by further evidence. The writer of the present article hopes to bring forth this evidence soon and wishes here to deal with the herringbone pattern only.

There are some instances of the herringbone pattern occurring in the ancient world which have not yet been connected with the Mesopotamian ones, so far as the writer's knowledge goes. Furthermore, not only the pattern, but also the stone, which is the original material for it, is used. The places are Troy, where it is common in the first settlement with an isolated example in the fifth,<sup>7</sup> the contemporary settlement at Thermi on Lesbos,<sup>8</sup> and Eleusis in Attica, the latter also being of about the same date.<sup>9</sup>

Is there connection between Mesopotamia and the western places? It is true that the natural shape of the stone material suggests very strongly the technique of putting the flakes obliquely so that a spontaneous origin at different places is possible. Furthermore, it cannot be excluded with certainty that the modern use of the herringbone pattern around Mosul and in the Anti-Lebanon might be a survival from ancient times, but it is hardly possible to connect the occurrence in Southern France of today with specimens from Roman times<sup>7</sup> and the latter with the ancient Aegæan ones. The matter is, however, different in regard to the Aegæan and the Mesopotamian examples. First, they belong to the same time, because we can say, in spite of uncertainties and divergences of

<sup>6</sup> *Loc. cit.*, 25 ff.

<sup>7</sup> Dörpfeld, *Troja und Ilion*, I, 47; Blegen, *AJA* XXXVIII, 1934, 225.

<sup>8</sup> W. Lamb, *Annual Brit. School Athens*, 30, 1928/9, 1929/30, 6.

<sup>9</sup> Mylonas, *AJA* XXXVIII, 1934, 226, 260.

<sup>7</sup> J. Darm, *Baukunst der Römer*, 2 ed., 212.

opinion, that a date at about 3000 shows them in use both in Mesopotamia and in northwestern Anatolia.<sup>8</sup>

Then, connections between Anatolia and Mesopotamia can be proved for these early times. Hutchinson may be correct in denying close connection between the Uruk-ware and western Anatolian pottery;<sup>9</sup> but that the Uruk-ware is related to Anatolian ware in general and that the pottery from Troy and Yortan belongs to this group must be accepted as certain.<sup>10</sup> Many parallels between Troy and Mesopotamia, enumerated by Hall and Childe,<sup>11</sup> as in the shapes of daggers, pins, jewelry and so forth, do not appear before the second settlement, it is true, and also the well-known female figure from Troy which shows Mesopotamian influence<sup>12</sup> is later, but the ribbed sherds from Therapi<sup>13</sup> indicating metal prototypes remind one of the fluted silver vases from the Royal Tombs at Ur dating from the "plano-convex" period.<sup>14</sup> An independent origin of the herringbone pattern in Mesopotamia and in Anatolia thus becomes unlikely.

A second problem is the direction in which the connection between Mesopotamia and Anatolia took place. As to Anatolia, there is no doubt about the way the influence came. Western influence from Greece, where the pattern also occurs, as we mentioned, is excluded, not because the Greek example is later, but because the spread of civilization from Anatolia into Greece has been proved beyond doubt.<sup>15</sup> An origin in western Anatolia is hardly likely,

<sup>8</sup> For the Mesopotamian chronology see Frankfort, *Archæology and the Sumerian Problem*, Chicago, 1932, 5 ff.; Herzfeld, *Arch. Mitt. Iran*, 5, 1933, 1 ff.; Albright, *AJA* XXXVIII, 1934, 608 f. For the chronology of Troy and Lesbos see H. R. Hall, *Civilization of Greece in the Bronze Age*, Chronol. Table; Reisner, *Antiquity* 1931, 200 ff.; Lamb, *loc. cit.*, 27. Aberg's attempt to lower the dates must be rejected, cf. Childe, *Antiquity* 1934, 369. Cf. also Frankfort, *Proc. J. Int. Congress of Prehistoric & Protohist. Sciences*, 169.

<sup>9</sup> *Iraq*, 2, 1935, 211 ff.

<sup>10</sup> Frankfort, *loc. cit.*

<sup>11</sup> Hall, *loc. cit.*, 54; Childe, *The Aryans*, 132, and *Mitt. Anthropol. Ges.*, Wien, 63, 1933, 221.

<sup>12</sup> V. Müller, *Frühe Plastik in Griechenland und Vorderasien*, Augsburg, 1929, 22.

<sup>13</sup> Lamb, *loc. cit.*, 10 ff.

<sup>14</sup> Woolley, *loc. cit.*, pls. 160-2, 170.

<sup>15</sup> Frankfort, *loc. cit.*, 52 ff. and *Studies in Early Pottery of the Near East II*, 96 ff.; Haley and Blegen, *AJA* XIX, 1928, 334 ff., and F. Matz, *Zeitschr. f. Ethnol.* 66, 1934, 429.



for this region was at that time not a center from which influences spread eastward. Eastern connections of Troy I are, on the other hand, ascertained by the presence of copper,<sup>16</sup> because the origin of metallurgy must be localized with great probability in the eastern highlands.<sup>17</sup> With these highlands, however, the Mesopotamia of the Early Dynastic period also had connection, as the many similarities between Mesopotamian metal objects and those of the Caucasus region, such as socketed axes and adzes, eyelet and spatula pins, "Spanish" combs, and others, show.<sup>18</sup> We are, therefore, justified in surmising a connection for other features also, that is in our case the herringbone pattern and the shape of the material used. Whereas it is a matter of dispute whether the types of the early metal objects originated in Mesopotamia and are importations in the Caucasus region or whether the opposite is true, we can safely assume that the shape of the plano-convex bricks and their arrangement in the herringbone pattern is an importation into Mesopotamia. Delougaz thinks that it originated in the foothills rising from the Mesopotamian plain, but a more distant origin in the highlands themselves is made probable by its occurrence in the West.<sup>19</sup> A common source between western Anatolia and Mesopotamia is thus most likely.

I refrain from drawing any historical conclusion from this single point. Only after the connections of the other constituent elements of early Mesopotamian art have been discovered will it be safe to solve the ethnic problems.

<sup>16</sup> Frankfort, *Studies* 2, 7; Lamb, *loc. cit.*, 35.

<sup>17</sup> Frankfort, *Studies* 2, 147 and *Archaeology*, 52 ff.; cf. H. Peake and H. J. Fleure, *The Corridors of Time* IV, *Priests and Kings*, 11 ff.; Speiser, *Excavations at Tapa Gavera I*, 167.

<sup>18</sup> Frankfort, 62 ff. Cf. Woolley, *loc. cit.*, 303 f.; Speiser, *loc. cit.*, 167.

<sup>19</sup> Speiser also is inclined to assume an origin not too close to Mesopotamia.

## THREE RARE MANUSCRIPTS FROM THE JOHN FREDERICK LEWIS COLLECTION

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THE JOHN FREDERICK LEWIS Collection of Oriental manuscripts and miniature paintings was very carefully selected by the late Mr. Lewis during his lifetime, and after his death was presented to The Free Library of Philadelphia in honor of his memory by Mrs. John Frederick Lewis. It contains 35 Arabic, 52 Persian, 10 Turkish, 3 Hindustani, 8 Sanskrit, 1 Nepalese, 4 Pāli, 1 Siamese, 11 Armenian, 1 Coptic, 13 Ethiopic, 10 Hebrew, 1 Samaritan, and 2 Syriac manuscripts, and 1 Egyptian papyrus.

The Lewis Collection is particularly rich from the standpoint of its manuscripts of historical value. I could not possibly attempt to enumerate and to comment upon the various manuscripts dealing with the history of Iran, India, and Turkey, most of which were written by contemporary historians of their times and formed part of the libraries of Oriental kings, as shown by their seals and memoranda. Nor shall I attempt in this paper to mention all of these by their titles or names. But I shall describe three important manuscripts in connection with which I have made special studies while preparing the catalogue of the collection.

### I

One of these (Fig. 1) which concerns itself with Turkish history, is a remarkably valuable manuscript, the "Last Will and Testament of Ahmed Pāshā," the Grand Vizier of Sultān Bāyezīd II, who reigned from 1481 to 1512, and of his successor Sultān Salīm I, who reigned from 1512 to 1520. Its binding is missing, its size is 10 inches by 6¼ inches, and it consists of 33 folios written in a large beautiful Thuluth hand, 7 lines to a page inclosed by gold-rulings. The name of the scribe is Muḥammed and the date given is 917 A. H. (1511 A. D.).

In 1481 Sultān Muḥammad, the Conqueror of Constantinople, died. His son Bāyezīd was called from Asia Minor to take the rule. His younger brother Jem organized an army to march to the capital in an attempt to seize the throne. But Bāyezīd arrived

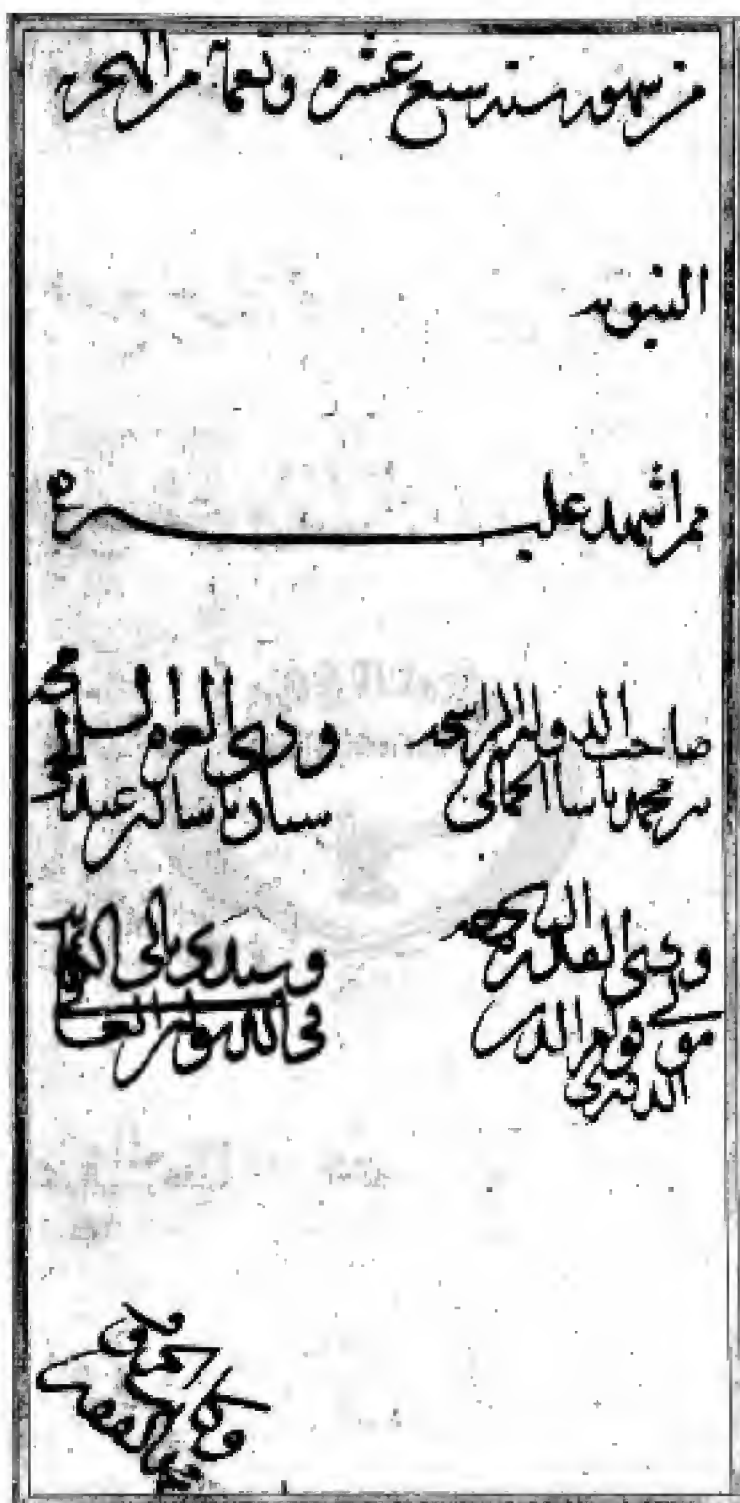


Fig. 1. The colophon of the Last Will and Testament of the Grand Vizier Ahmed Pasha

first, assumed control and sent out an army to defeat his brother. Our Ahmad Pāshā of the manuscript first comes into the pages of Ottoman history when he was made the commander of the army sent to subdue Jem. He tried to arrive at a peaceful settlement but failed. Bāyezīd, impatient, led reinforcements to aid Ahmad Pāshā, and Jem, faced with this overwhelming force, fled to Egypt, where he was well received by the Memluk sultan.

Since the Egyptian sultan had befriended his brother, Bāyezīd organized a campaign against him, and Ahmad Pāshā was placed in command of this expedition. He was taken prisoner, and Bāyezīd sent a second army against Egypt, which forced the Egyptians to come to terms and to release Ahmad Pāshā. Ahmad Pāshā then returned to Constantinople, and was made the commander of the fleet, which post he retained for five years. He was later appointed grand vizier, a position previously held during Bāyezīd's reign by fourteen others, thirteen of whom ended their careers by being either beheaded or imprisoned.

In 1497, a year after his appointment as grand vizier, war was declared against Venice, and he was deprived of office because the sultan feared he might be too sympathetic towards the Christians and his former relatives, as he had been a Christian before he became a Muslim. Later from 1503 to 1506 he was made grand vizier for the second time, in recognition of his services in connection with a peace treaty which he concluded with Hungary. In 1509 there was a great earthquake in Constantinople. The tremors continued intermittently for forty-five days, and the whole country was terrified. The sultan himself encamped in the garden of his palace. Walls of the city cracked and fell into pieces; 109 mosques, 1610 houses, and thousands of persons perished. Many villages in Asia Minor were destroyed. Dervish Muhammad, a contemporary Turkish historian, mentions that Ahmad Pāshā had come to the rescue of the poor and horror-stricken people by handing out money and providing food to the destitute.

Two years later, in 1511, he became the grand vizier for the third time, but he narrowly escaped being killed during the revolts of the Janissaries, who were claiming Salīm, the youngest son of the sultan, as their king and were demanding that Bāyezīd resign in favor of Salīm. Ahmad Pāshā was the only one to advise the sultan to resist this undignified demand. Bāyezīd followed Ahmad Pāshā's advice,

but the army and the opposing party were strongly against him, and he finally resigned in favor of Salim, who triumphantly entered the capital. Ahmad Pāshā was naturally forced out of his office and was fortunate to escape with his life. Salim's reign lasted until 1520. During this time there were two important campaigns, one against Iran and the other against Egypt. Salim was victorious in both and Ahmad Pāshā, whose ability had been recognized by the young sultan, was asked to accompany him in both these campaigns and received responsible commands. He was made grand vizier by Salim, but resigned after a year, and later refused a second offer on the grounds of ill health. It was during his return from the Egyptian campaign in 1516 that he died, according to the German historian von Hammer, or was beheaded by the enraged sultan, according to Richard Knolles. Knolles wrote a history of Turkey in 1611, that is to say, about one hundred years after this event; the work seems to be authentic as it agrees with the writings of most of the contemporary Turkish historians. I shall quote both these authorities before ending this story. Knolles calls Ahmad Pāshā "Cherseogles," evidently a corruption or mispronunciation of *Hersek-Oğlu*, the name given to him by Turkish historians.

"But now that we have fallen into the remembrance of this Cherseogles, it shall not be amiss, both for the honour of the man, and the great love he always bore unto the Christians, to step a little out of the way, to see the cause why he being a Christian born, turned Turk. For he was not (as almost all the rest of the great men about Bajazet were) of a Child taken from his Christian Parents, and so brought up in the Mahometan Religion; but now being a man grown, turned Turk; yet so, as that he never in heart forgot either the Christian Religion or love towards the Christians; a thing not common among such Renegates. He being the Son of one Cherseogles, a small Prince of Illyria near unto the Black Mountain; and going to be married unto a Lady whom he most entirely loved, and unto whom he was already betrothed, honourably descended of the House of the Despot of Servia; his intemperate Father with lustful Eye beholding the young Lady of rare Feature and incomparable Beauty, desired to have her for himself; and regarding more the satisfying of his own inordinate desire, than his own honour, or of the Fatherly Love of his Son, took her in marriage himself; all his Friends laboring in vain to dissuade him, and with open

mouth crying shame of so foul a Fact, wherefore the young man, moved with the dignity of so great an injury, and driven head-long with despair, fled first to the Turks Garrisons which lay not far off, and from thence to Constantinople; where the fortune of the man was to be wondered at; For being brought before Bajazet, who with cheerful countenance entertained him, for that he was honourably descended, and well liked both of the man, and of the cause of his revolt; smiling upon him, said, 'Be of good cheer Noble Youth, for thy great courage is worthy of far greater fortune than thy fathers House can afford thee; now in stead of the Love wrongly taken from thee by thy Father, thou shalt have given thee in marriage the Daughter of a great Emperor, of rare and singular perfection.' And long after abjuring his Religion, and changing his name of Stephen to Achomates (Ahmad) and Cherseogles, (Hersek-Ogla) he married one of Bajazet his Daughters, a Princess of great Beauty; and deserved to have a place amongst the Bassas (Pashās) of great honour in the Court."<sup>1</sup>

Von Hammer, however, quoting Awliyā Chalabī, the Turkish historian, speaking of the return from Egypt says:

"... et non loin d'Alep mourut le vesir Herseck-Ahmed-Pascha, fils de duc de Saba Etienne-Cossarich, qui, pendant un demi-siècle, avait rendu tant d'importants services à l'empire ottoman, et avait occupé quatre fois la première dignité de l'Etat. Sa mémoire est conservée par la mosquée et la cuisine des pauvres subsistant encore aujourd'hui, qui furent construites à la pointe la plus extrême du golfe de Nicomédie, sur le rivage méridional."<sup>2</sup>

Both von Hammer and the manuscript refer to the important charitable works of Ahmad Pashā. The manuscript is his last will, and according to it he left all his wealth and huge estates to charity. The will directs the disposition of funds for the erection of mosques and the care and the feeding of the poor villagers. Whole villages belonged to him, mostly inherited from his wife, the sultan's daughter. These were situated both in Asiatic and in European Turkey, and some of them are still known by the names set forth

<sup>1</sup> Richard Kœlles, *The Turkish History from the Origin of that Nation*, 1. 329, London, 1887.

<sup>2</sup> P. J. Hammer, *L'Histoire de L'Empire Ottoman*, French Translation by Dochez, 1. 457-458, Paris, 1844. For further particulars see Hammer, *Geschichte des Osmanischen Reiches*, 2. 264 ff., and 526-27, Pesth, 1827-1835.

in the will; the income from these is given to carry out the purposes of the will. Administrators of proven piety and integrity are named, and their salaries are fixed, and even the number of sheep to be slaughtered daily and loaves of bread to be baked for the poor are enumerated.

On the colophon of this manuscript, besides the name of the scribe and the date, the names of Sinân Pāshā, and Piri Pāshā, the two other grand viziers of the time, appear as witnesses. There are two other names which so far have not been identified.

## II

The second manuscript (Figs. 2, 3) deals with the history of Iran. It is a document written by the order of Sulṭān Ḥusayn Ṣafarī, acknowledging a bequest in Isfahan. The volume is 9 inches by 6 inches and consists of thirteen folios by the famous calligrapher Muḥammad Bāqir al-Ḥusaynī. The first page and the binding are missing, but the document has been rebound in modern English red levant morocco.

Sulṭān Ḥusayn reigned from 1694 to 1721. In 1721 Maḥmūd, an Afghan chief, marched against him, defeated his army, and besieged the capital city Isfahan. Shah Ḥusayn's son Ṭahmāsp, in the meantime, deserted his father and escaped to the mountains of Māsanderān. The shah on account of famine offered to capitulate. Maḥmūd entered Isfahan in triumph, and seated himself on the throne in the royal palace. In 1723 Maḥmūd invited three hundred of the principal Persian nobility to a banquet and murdered them. To prevent their children's attempt at vengeance he also murdered all of them. Not content with this, in 1725 he assembled all the captive members of the royal family, except the shah and the queen, in the courtyard of the palace, and caused them all to be murdered. At length Maḥmūd died in that same year; he was succeeded by his cousin Aṣḥraf. In 1729 Ṭahmāsp, the fugitive prince, assisted by Nādir Khān, who later became the shah, assembled an army and marched against Aṣḥraf. Aṣḥraf alarmed led an army against the prince, but was defeated and fled through the town, not forgetting to murder the poor old Sulṭān Ḥusayn. Shāh Ṭahmāsp entered triumphantly into Isfahan.

Now this manuscript in the Lewis Collection, which makes a be-





أَسْمَعُ بِأَلْفِ كَلِمَةٍ فِي الْمَقْدَسِ عَلَى

مَهْجَرِهِ لَا تُؤْخِرُ الصَّلَاةَ وَالْجَنَّةَ

مَعَهُ

بِهِمْ كَيْدُ مَنْزِلِي شِدْ وَفَقْدُ



Fig. 4. Page from the Bequest of Sultan Husayn  
Safavi bearing seals of witnesses

بِهِمْ كَيْدُ مَنْزِلِي شِدْ وَفَقْدُ

مَعَهُ

بِهِمْ كَيْدُ مَنْزِلِي شِدْ وَفَقْدُ

بِهِمْ كَيْدُ مَنْزِلِي شِدْ وَفَقْدُ



Fig. 2. Page from the Bequest of Sultan Husayn  
Safavi bearing his seal



quest to the city of Isfahan by Sultān Ḥusayn, sets aside what, in effect, was a funeral parlor for the needy Muslims where they might wash and prepare the bodies of their dead for proper burial, in full accordance with the requirements of Islam. It is stipulated in the document that the building and its equipment may not change hands, may not be sold, and should be free to the poor forever.

Each page of the manuscript is gold-ruled and the sultan's name whenever mentioned appears in gold. Each page also bears the seal of one of the shah's sons and that of the Chief Justice, and of two other officials whose positions have not yet been identified. The date of the manuscript is 1118 A. H. (1706 A. D.), or twenty-three years before Sultān Ḥusayn's death.

### III

The third manuscript (Fig. 4) concerns itself with northern India. It contains a poem in praise of 'Alī, the fourth caliph, by the Persian poet Mullā Tughra who lived during the seventeenth century. It is 7 inches by 5 inches and consists of 12 folios of gold-sprinkled paper written in excellent Nasta'liq style. It bears the seal and the handwriting of Jān Sipār Khān, the Turkoman Amīr, a contemporary of the last of the Mughal emperors Aurangzib. Above the large circular seal impression, in large *Shikastah* style, appear these words in Arabic: "From the most humble slave," and then the seal impression below it reads: "Jān Sipār Khān, Khānah-Zād-i- (a slave born in the house of) Shāh-i-'Alam (which was the title of Muḥammad Mu'azzam, the second son of Aurangzib) Pādishāh-i-Ghāzī (the victorious king) 1121 A. H. (1709 A. D.) Sanah 3 (the third year of the reign of the emperor)." Underneath the seal a little to the right, written again in large characters appears the word "Hū" (pronounced as *hu*, but written *hūwa*), which refers here to the God Almighty, and means "He (is) or He (exists)," but is more generally used as a substitute for the well-known Qur'ānic formula: "In the name of the Most Merciful God." Then follow four lines in Persian poorly written in small Indian *Shikastah* which read: "From the possessions of the humble slave Jān Sipār Khān, the son of Rustamdilkhān, the son of Jān Sipār Khān al-Ḥusayn al-Mukhtār al-Sahzawārī, may the Lord forgive his sins and cover his misdeeds. From the gifts of Nawwāb (prince) Shāhib (sir, master) and Qiblah (another title) Nawwāb

Mukhtār Khān to whom may God manifest His proof. Saah 1123 A. H. (1710 A. D.)."<sup>2</sup>

Jān Sipār Khān was the second son of Mukhtār Khān Sahzawārī, an Amīr during the reign of Aurangzīb. At the time of his death in 1701 he held the sūbahdārī, or the Governorship, of Hyderabad. The grandfather of Jān Sipār Khān, who bore the same name, was an Amīr with an army of four thousand men during the reign of Emperor Jahāngīr. He was appointed Governor of Allahabad in the first year of Shāh Jahān, 1628 A. D., and died there during the same year.<sup>3</sup>




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<sup>2</sup> The traditional phrase *أَفَارَ اللَّهُ رَحْمَةً* has been translated by Lane in his Arabic lexicon as: "God taught him His proof." *أَفَارَ*, the fourth form of a hollow verb, is Active Perf. 3 s. m., and should therefore have been translated as: "has taught," or rather, "has enlightened." But in such a connection as that of our text this phrase is employed as a wish or a blessing upon the deceased.

<sup>3</sup> See *An Oriental Biographical Dictionary*, by T. W. Bask. Revised and enlarged by H. G. Keene, London, 1884.

# SANSKRIT *pārśu* AND *parśu*

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FROM THE beginning of Vedic research there has existed confusion<sup>1</sup> between the two words *pārśu* "rib," later also a sort of sacrificial sickle, and *parśu* "ax," due largely to the similarity of the words themselves and to the fact that occasionally in later literature *parśu* was, *metri causa*, actually contracted to *pārśu*.<sup>2</sup> This confusion is evident only in Sanskrit, not in Indo-European, since *parśu* is of course equivalent to the Greek *πάρσις*, going back to an IE. \**peleku*, borrowed from the Akkadian *pilaqu* (from Sumerian *belag*). *Pārśu* is IE. \**perku*, whence Avestan *perəuš* "rib," "side," Ossetic *fars* "side," and Old Church Slavic *prāsi* "breasts." This note is an attempt to distinguish and explain the two Sanskrit words once and for all.

*Pārśu* occurs uncompounded in the Rig-Veda four times, twice (S. 6. 46 and 10. 84. 23) unquestionably as a proper name. In the latter passage "the daughter of Manu, *Pārśu* (i. e. Rib) by name" suggests an interesting parallel to the Biblical story of the creation of Eve. Twice (in 1. 105. 8, repeated in 10. 33. 2) *pārśavak* means simply "ribs." The latter passage reads, "from all around ribs (*pārśavak*) give me pain like co-wives." Whether "ribs" here refer to the walls of the well in which Trita was confined, as Sâyana and the Nirukta (4. 6) take it, or to the ribs of the sufferer himself need not concern us. Most modern commentators and translators favor the interpretation of a needy poet suffering from hunger.<sup>3</sup>

In the Atharva-Veda, *pārśu* means simply "rib" in 9. 7. 6, 10. 9. 20, 11. 3. 13, but it occurs with the meaning "sickle" in

<sup>1</sup> As evidence of this confusion, cf. Sâyana, below, and Macdonell and Keith's *Vedic Index of Names and Subjects*, I, 503, where *pārśu* "sickle" is mistakenly explained as "being apparently a variant of *parśu*."

<sup>2</sup> *Rāmāyaṇa* 3. 28. 24: *prācān pāśān tathā parśān kūtīkān ca kupaṇḍān tathā*; cf. also *Trikūṇḍinīya* 2. 8. 25, *Harivansha* 5970, etc.

<sup>3</sup> Cf. Geldner *Der Rig-Veda* I, 124; Grassmann's translation II, 471; and especially Bloomfield, *Rig-Veda Repetitions*, p. 114. Ludwig erratically considers it (*Der Rig-Veda* II, 576) a proper name.

7.28.1 and 12.3.31. The latter passage might be rendered as follows: "Stretch forth the sickle (*pāśū*) speedily, take (it) with haste; unharming let them cut the plants at the joint." In 7.28.1, when compared with the obviously parallel passage in the Taittirīya Saṁhitā 3.2.4, we find a striking example of the confusion, already mentioned, which grew up between *pāśū* in its secondary meaning "sickle" and *paraśū* "ax." The A.V. passage reads: *vedaḥ svastir druḡhaṇaḥ svastiḥ pāśāś varāś vedīḥ paraśār naḥ svasti*, "the sacred grass is well-being, the tree-slayer is well-being; the sickle is sacrificial altar, the ax is our well-being." The chiasmic order here makes it evident that the peculiar form *pāśāś* (with accent on the first syllable) is equated with *vedaḥ* (hence *pāśū* "sickle" for cutting sacred grass) and *druḡhaṇaḥ* "tree-smiter" is equated with *paraśū* "ax." This interpretation is further verified, first, by the fact that the commentator understands for this *pāśāś* the word *pāśū* "sickle," and, second, by the above-mentioned passage from the Taittirīya Saṁhitā: *spṛyaḥ svastir viḡhaṇaḥ svastiḥ pāśār vedīḥ paraśār naḥ svastiḥ*.<sup>4</sup>

We have seen, then, that in the Rig-Veda *pāśū* means only "rib," but that it came also to mean "sickle" in the Atharva-Veda. This semantic transfer becomes instantly clear when we take into consideration the rib-like shape of the instrument and realize that the rib of a horse was actually sharpened to make a sickle.<sup>5</sup>

There remains, however, a compound of *pāśū* in the Rig-Veda which has been consistently misinterpreted since Sāyana. In 7.83.1, a hymn addressed to Mitra and Varuna by the priests of the victorious king Sudās, we read: *yuvām namā pāśyamānāsa āpyam prācā gavyantaḥ prthuparśavo yayuḥ*. We translate this: "Beholding you and your friendship, oh men (Mitra and Varuna), they went forward, eager for booty, broad-chested." Sāyana comments on *prthuparśavaḥ* as follows: *prthuparśavaḥ prthur vistirṇaḥ parśuḥ parśvasṭhi yeṣām te lathaktiḥ vistirṇāścuparīṇastāḥ*, that is: "*prthuparśavaḥ*: *prthu* 'broad,' *pāśū* 'rib-bone'; so spoken

<sup>4</sup> *viḡhaṇaḥ* "wooden pestle" is here substituted for *druḡhaṇaḥ*, since the passage deals with the Soma sacrifice; cf. Bloomfield ZDMG 48, 646. On both passages cf. the Whitney translation of Atharva-Veda, pp. 407, 408, and Keith's *The Veda of the Black Yajus School*, 1, 242.

<sup>5</sup> Cf. Sāyana, quoted below, and Nirukta 4.3 where the "side" (*pārśvāṁ*) is spoken of as "shaped like a sickle" (*pāśū-mayam*).

of because they had the broad ribs of horses in their hands." Among those who favor Sāyana's view should be noted particularly Böhtlingk and Roth ("breite Krummsäbel, oder Hippen tragend")<sup>8</sup> and the translations of Wilson, Grassmann, and Hillebrandt. But since this is a battle hymn, we are confronted with the obvious difficulty of arming our heroes with agricultural or sacrificial implements. Realizing this, Grassmann translates the compound "mit breiten Säbeln," while Zimmer, Geldner, and Griffith prefer a translation "with broad axes."<sup>9</sup> This latter interpretation is untenable since, in the first place, *paraśu* "ax" is apparently not contracted to *pārśu* until epic times, and, secondly, because in the Rg-Veda *paraśu* is used only to designate the ax of the wood-cutter. For example, in 7. 104. 21 we read "like an ax, Śakra, that splits the timber," and again in 10. 28. 8, "the gods came bearing axes and cutting the woods with their followers."<sup>10</sup>

Ludwig again differs by translating *prthuparśavaḥ* as a dvandva compound, "the Pythas and the Parśus," and the same view is held by Weber.<sup>11</sup> That the subjects of this Vedic hymn should be extraneous tribes, whether Parthians, Persians, or the Parśus known to Pāṇini, is impossible and this view has been adequately refuted by Zimmer.<sup>12</sup>

The translation which has already been suggested for *prthuparśavaḥ*, namely "large-ribbed," "broad-chested," "strong"<sup>13</sup>

<sup>8</sup> The curved sword, or schmitz, was apparently unknown in India before epic times.

<sup>9</sup> Grassmann, *Rig-Veda*, II, 305; Zimmer, *Alt-Indisches Leben*, p. 134 ff.; Geldner, *Siebenzig Lieder des Rig-Veda*, p. 32, and Wilson, *Hymns of the Rig-Veda*, II, 79.

<sup>10</sup> Cf. also RV. 1. 127. 3, 9. 3. 4, 9. 67. 30, 10. 42. 9, 3. 53. 22, 4. 6. 8, 10. 53. 9, 1. 130. 4, 5. 49. 4.

<sup>11</sup> Ludwig, *op. cit.*, V, 547 and Weber, *Indische Studien*, IV, 378.

<sup>12</sup> *Op. cit.*, pp. 433, 434.

<sup>13</sup> We had already reached this conclusion when we found that Macdonnell and Keith in their *Vedic Index of Names and Subjects*, I, 504, suggest this as a possibility on the authority of Roth following Sāyana (reference not given for either). We have seen above Sāyana's exegesis, and Roth nowhere suggests "with large ribs." Apparently Macdonnell and Keith misread the St. Petersburg Lexicon where a few lines above the entry *prthuparśu* Roth has "*prthuparśu*, nach Sāj. adj. 'breite Seiten habend,'" used for sides of a wagon in RV. 8. 26. 23. We later noted that Geldner in *Rig-Veda in Auswahl*, I (Glossar) renders *prthuparśu*, "mit weiter, ge-



appears now as the only true meaning after an examination of the history of *pársu* and *paraśú*. The compound corresponds to a host of others, such as *pr̥thu-vaksāḥ* "broad-chested," *pr̥thu-vāṇas* "broad-shouldered," and *pr̥thu-pāṇi* "broad-handed," i. e. "generous." Compare also Lithuanian *píršingas* (Geitler) "breithrústig," from *píršas* "breast" (cf. Old Church Slavic *prəsi*, above).




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hobener Brust," but this has borne no fruit since it has not appeared in any translation, by him or any one else.

## BRIEF COMMUNICATIONS

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### *The Illuministic Sufis*

THAT Sufism is a way rather than a set of disciplines and doctrines has now been established.<sup>1</sup> Yet the fact that the early Moslem mystics sought support for their views in the Koran<sup>2</sup> can hardly annul the concept that theirs was a psychological behavior engendered as a direct response to the all but human aspiration for personal approach to the Deity and a spiritual contact with religious truth. In Arabic literature the form *sūfi* first appears in the middle of the ninth century when it was applied to a certain class of ascetics. But Moslem ascetics must have lived before that time.

Syria in pre-Islamic days saw the rise of the three-staged mystical experience—Purgation-Illumination-Perfection. For in the sixth century Dionysius, a Syrian monk, developed a mystical theosophy based on Hellenistic sources in the main.<sup>3</sup> But the first mystics of Islam, it must be conceded, were only interested in the first of these three stages, that is, Purgation. Gradually, however, they began to develop the second stage, that is, Illumination. Al-Muḥāsibī<sup>4</sup> who pioneered with his disciples in the pathways of Purgation was one of the first to declare that as purification brings freedom from the attachments of this world one might expect to attain the stage of Illumination and thence proceed to the unitive life with God.

Ibn-Masarraḥ of Cordova (A.D. 883-931) founded the Illuminis-

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<sup>1</sup> D. B. Macdonald, "The Unity of the Mystical Experience in Islam and Christendom," in *The Moslem World*, 25, No. 4 (Oct. 1935), 325-335.

<sup>2</sup> Such passages as Koran 4: 96, 9: 113, 33: 47 must have served them in good stead. When Sufism was passing through a crisis and especially after the execution of al-Hallaj (A.D. 992) was in the danger of being outlawed, the deeply concerned Sufis sought to make their system conformable to the strictest standards of orthodoxy. See A. J. Arberry, *The Doctrine of the Sufis*, Cambridge, 1933, pp. xiv, xv.

<sup>3</sup> Margaret Smith, *Early Mysticism in the Near and Middle East*, London, 1931, p. 79.

<sup>4</sup> Abū-Abdullāh Ḥārith ibn-Asad (A.D. 731-857), was born at al-Baṣrah and taught in Baghdad. See Margaret Smith, *An Early Mystic of Baghdad*, London, 1936. Professor Massignon describes him as "the true master of primitive Islamic mysticism."

tic (*Ishrāqī*) and pseudo-Empedoclean school.<sup>8</sup> Later on an essential element of the *Ishrāqī* teaching—the metaphysical doctrine of light—reappears in the Divine Comedy.<sup>9</sup> Here we find, reproduced a century after *Ibn-ʿArabī*,<sup>10</sup> most of the pictures the latter used of the realms beyond the grave. Seen in this perspective Dante would pose as one of the ardent adherents of the Illuministic school. In the main, it may here be stated, the doctrine of the Illuministic Sufis was based on a spiritual philosophy with a mystical theory of knowledge. God and the world of light were interpreted as radiation and our process of cognition as illumination from above.<sup>11</sup> The system was consummately referred to as *ḥikmat al-ishrāq* (wisdom of Illumination), Abu-al-Futūḥ al-Suhrawardī<sup>12</sup> and his colleagues being largely associated with it. They were undoubtedly steeped in the syncretic philosophy of Hellenism which reached the Hither Orient in the form of neo-Platonic, Hermetic,<sup>13</sup> and allied sources, and was there woven with Persian and other speculations.

A manuscript in the Bārūdī Collection, acquired by Mr. Robert Garrett of Baltimore in 1925,<sup>14</sup> entitled *Risālat Qawānīn Ḥikmat al-Ishrāq ʾilā Kull al-Ṣūfiyyah bi Jamʿ al-ʿĀfāq* (A Treatise on the Articles of the Maxims of Illumination addressed to all the Mystics of the World), by abu-al-Mawāhib al-Shādhilī (died A.D. 1477/8), harks back to the works of al-Suhrawardī and *Ibn-ʿArabī*. The author belonged to the Shādhilī fraternity of Sufis, founded by

<sup>8</sup> Miguel Asín Palacios, *Islam and the Divine Comedy*, tr. Harold Sunderland, London, 1920, p. 204; while no great champion of al-Ishrāq is recognized in the Iberian Peninsula before *Ibn-Mazzarrah* it might still be more accurate to accord him the honour of being the founder only in Spain of this movement.

<sup>9</sup> *Ibid.*, pp. 164-165; also consult the same author's *Abu-masarra y su escuela*, Madrid, 1914, pp. 120-121.

<sup>10</sup> Abu-Bakr Muḥammad *Ibn-ʿAlī* Maḥyī al-Dīn al-Ḥātini al-Tāʾi, born at Murcia A.D. 1165, died in Damascus 1240.

<sup>11</sup> Hermes, Agathodæmon, Empedocles, Pythagoras, and Plato were prominent authorities often described as prophets or inspired sages.

<sup>12</sup> Aḥmad *Ibn-Ḥabash* *Ibn-Amrāk* Shihāb al-Dīn, A.D. 1164-1191; born at Suhraward, Persia, lived and studied at Marāghah, Isfahān, Baghdād, and finally Aleppo, where he was murdered.

<sup>13</sup> Consult T. J. de Boer, "Urānī," in *ZA* 27 (1912). 3-15.

<sup>14</sup> Now deposited at Princeton University Library. A Catalogue of the entire Garrett Collection of Arabic MSS is in press.

abu- al- Hasan 'Alī ibn-'Abd al-Jabbār al-Shādhilī<sup>12</sup> who was born at al-Shādhilāh near Jabal Zafrān in Tunisia.<sup>13</sup> It would seem that he had based his doctrines on a foundation introduced into Morocco and North Africa by the Sevillian teacher of ibn-'Arabi, the so-called abu-Madyan. Such men as 'Abd al-Salām ibn-Mush'ish, who was the master of the founder of al-Shādhiliyyah, as well as 'Abd al-'Abbās al-Mursī and ibn-'Abbād of Ronda are great Shādhilī thinkers who became legitimate heirs of the Illuministic trends of ibn-'Arabi.

The author of the MS referred to above lived in Cairo during the last century of the Mamlūk period. His work represents one of the very few treatises written by Sufis on this fascinating phase of Moslem mystical life. It opens two vistas before our vision: the one leading into the depth and mystery of the Illuministic school of thought, the other disclosing the birth and rise of Moslem Sufi fraternities of which al-Shādhiliyyah—the author's own order—is a unique organization whose influence was unexcelled in the annals of Moslem mysticism.

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### *The Parts of a Vina*

I have discussed the terminology of the parts of the old Indian postless harp in JAOS 50.244, and 51.47 and 284.<sup>1</sup> Jaiminiya Brāhmaṇa II. 69-70 affords additional information. As the text and German version by Caland, *Das Jaiminiya Brāhmaṇa in Auswahl* (Amsterdam, 1919), p. 143, are not very accessible, an English rendering, with brief comment, is given here:

"The Progenitor (Prajāpati) and Death (Mṛtyu) were sacrificing, in opposition to one another. The sacrificial utensils were those that are now used in Iṣṭi sacrifices. What was sung as the laud (i. e. by the Ūdgātṛ), or recited as formulae (i. e. by the Hotṛ) or

<sup>12</sup> Died A. H. 650/A. D. 1258.

<sup>13</sup> 'Alī al-Dīn al-Yāzī, *Kitāb Mir'āt al-Jawād*, Cairo, A. H. 1232, vol. IV, p. 146.

<sup>1</sup> See also "A passage on vina-playing," *Zeits. für Vergleichende Musikwissenschaft*, III, p. 88, 1035 (1935).

done (i. e. by the Adhvaryu), that was the Progenitor's party. But what was sung to the harp, or enacted (*ṛtīyate* 'danced'), and done, by way of mere entertainment (*vrīkṣā*), that was Death's party.<sup>2</sup> These two parties were now equally matched; one was as great as the other. For a long time, for many years, neither got the upper hand. Then the Progenitor willed, 'May I overcome Death!' He saw the perfect-form (*sampadām*) and due-measure (*samkhyānam*) of the sacrifice;<sup>3</sup> therewith he overcame Death. When he (Death) was overcome by these, he grieved; falling down hitherward, he entered the gynaeceum (*patnīśālām*), wives (*patnīḥ*) accordingly. . . .<sup>4</sup> What had been his lauds, his recitative, are now what people sing to the harp, or enact, or do to please themselves (*vrīkṣā*); his sacrificial stake (*yūpā*) is now the neck (*danḍa*) of the harp; his tie-ropes (*raśmāḥ*) are now the bindings (*upavīṇā*) of the harp; his soma-vessel (*dhruva-kalāśa*) is now the basket (*śāṇḍā*)

<sup>2</sup> It is, of course, *vrīkṣā*, not *ṛtīyate*, that carries the pejorative implication. Without reopening here the whole problem of "Mysteries and Mimicry in R̥gveda," it may be observed that RV. X. 72. 6 *ṛtīyate* *iva* affords an authority for "dancing" in the ritual, which it is well known is modelled "on what was done by the Devas in the beginning," and that Kaṣṭhaki Brāhmaṇa, XVII. 8 speaks of the sacrificing priests as "dancing," or rather as we should understand, "emulating" (*visaradyantī*).

<sup>3</sup> JB. II. 73 explains that the *samkhyānam* is what is immortal (*amṛtam*) in the sacrifice, the *asamkhyānam* what is deathly, or mortal (*mṛtyum*). *Samkhyānam* is apparently tantamount to *pramāṇam* and might be rendered by "ordinary" (n. as in "Ordinary of the Mass"), with "inordinate" or "irregular" as antithesis. As in BG. there is no reference to the philosophical system. *Samkhyā* in BG. would appear to be the "way of sacrifice" as distinguished from the "way of works." If *sampadā* and *samkhyā* are the "perfection and due measure" of the sacrifice, it is easy to see how *samkhyā* may have meant "one expert in sacrifice," or "one who performs the sacrifice aright, or correctly"; and it may be in this sense that the Vedic ṛṣi Atri is *samkhyā* in the Anukramanā.

<sup>4</sup> The rest of the sentence I cannot understand, but the sense is evidently that the music of Death became the pastime of women. Cf. ŚB III. 2. 4, where the mundane Devas, desiring to please her (*pramodayiṣyāmaḥ*) succeed in seducing Vāc, the Gāyatrī, by means of the music of harp and voice; from the Gandharvas' lauds and praises she turns away to the dancing and singing (*ṛtīyate śītā*) of the Devas; "and it is because Vāc thus turned, that right here and now (*īta-ā*) women are given to vanities [*mogha-samāśā*], for it was thus that Vāc turned thereto, and other women do as she did. And hence it is that to one who dances and sings they take a fancy most readily."

of the harp; his soma-pressing skin (*adhiṣaṃyā carman*) is now the parchment (*carman*) of the harp; his perforations (*uparava*) are now the openings (*ākāśa*) of the harp; his pestles (*grīṣaṇa*) are now the plectra (*vāḍana*) of the harp; his seven metres (*chandas*) with fourfold increment<sup>2</sup> are now the strings (*tantrī*) of the harp; his ten-nights lauds (*dāśarātra*) are now the ten fingers (*daśāṅgulī*). It was because the sacrifice was twofold that he came to grief; the sacrifice is really one, the sacrifice is just the Progenitor himself."<sup>3</sup>

The analogies enable us to clear up a few points that have remained obscure. For *upavāna* we suggest the emendation *upavāṇa*, or at any rate, that *upavāṇa* and *upavāna* are synonymous; the analogy makes it evident that the reference is to some kind of "bindings," and we suppose this to be the binding at the junction of the neck and belly, plainly shown in many of the representations. *Sūnā* is seen to be synonymous with Skr. *ambhāṇa* or *bhāṇḍa*, and Pali *doni*, *donikā*; <sup>4</sup> and *ākāśa* with *chakra*. *Chandas* appears to be synonymous with *svara*. Caland. (L c., p. 144) may be right in thinking that the "ten fingers" are simply the ten fingers, all of which are obviously made use of in one way or another in playing; in which case *angulimigraka* may mean, in quite the modern sense, "fingering."

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<sup>2</sup> For the meaning of this see 4B X. 3. 1. 1.

<sup>3</sup> I cannot see why Caland says that the last sentence is "nicht deutlich."

<sup>4</sup> *Sūnā*, "basket," may imply that the belly of the harp could be made of basket-work rather than wood.

## REVIEWS OF BOOKS

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*Subartu. Beiträge zur Kulturgeschichte und Völkerkunde Vorderasiens.* Von ARTHUR UNGNAD. Berlin und Leipzig: WALTER DE GRUYTER & Co., 1936. xi + 204 pages. RM 10.

Ever since the Amarna letters became known, historians have been aware of the fact that there existed in the middle of the II<sup>nd</sup> millennium B. C. in Upper Mesopotamia a state of great power, called the kingdom of Mitanni. Its king, in his correspondence with the pharaoh, employed a language which, though written in the usual cuneiform characters of the period, proves to be individual. Certain words of this language recur in vocabularies and are attributed by their ancient compilers to the language of Subartu.

The fortunate discovery of the Hittite archives has contributed to our knowledge also of this language and this nation; there the respective people are called Hurrites. This term is used also in the Amarna letter which is composed in the "Subartu" language and may, therefore, be taken as a native designation.

Archaeology has so far to only a relatively small extent devoted itself to the Hurritic problem. The excavations of Baron von Oppenheim at Tell Halaf are undoubtedly its main contribution. They have produced particularly interesting sculptures and a large amount of painted pottery.

The interpretation of the complex facts has already caused much discussion.<sup>1</sup> Ungnad's book is an original and important contribution to the subject. The author sets forth the thesis that the culture of Tell Halaf, characterized by the sculptures and the painted pottery, belongs to the Subareans whom he considers the basic population of Upper Mesopotamia since primitive times. He, then, shares

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<sup>1</sup> The author announces in his preface (p. v) that he could not take into consideration the pertinent publications which have appeared since 1933. This is very much to be regretted, particularly for the reason that with Herzfeld's, *The Tell Halaf* (1934) an exception has been made. Remarks like those on p. 187 (middle of the page) are out of place because A. Mourigat in *Die bildende Kunst des Alten Orients und die Bergvölker* and the reviewer in *Hebriter, Churriter und Assyrer* have treated the subject in 1933 and 1936 respectively.



and defends the opinion of the excavator of Tell Halaf, which has been eloquently advocated also by E. Herzfeld; he also opposes and wants to refute all those who make the Hurrites (Subareans) enter Mesopotamia not before the 20th century B. C.

The book is divided into three parts. An introduction deals with the racial history of the Near East. The author stresses the fact that in Upper Mesopotamia a race dominates which is characterized by a flat occiput ("Steilschädel"). Its territory in the Near East coincides with that covered by painted ceramics ("Buntkeramik"). The two features, according to Ungnad, are to be coördinated. A convenient term for this whole territory is looked for and found in the Subartu of the Akkadian inscriptions.

The second part of the book, therefore, collects all references to the country Subartu and its inhabitants, the Subareans, from the cuneiform documents. It gives full information also on the extant linguistic evidence such as glosses and proper names. This part of the book is of value regardless of the attitude that may be taken towards the author's main thesis.

The third chapter turns to the interpretation of the collected evidence. It reaches the conclusion that Subartu, attested since the Akkad dynasty (middle of the IIIrd millennium), extended from the Zagros and Western Persia in the East to Syria and the Mediterranean coast in the West; the Hurrite confederacy of the IIrd millennium, placed in the foreground by other scholars, is merely an episode in the long and significant history of the Subareans. The parallelism between the philological evidence and the archaeological and anthropological facts, in Ungnad's opinion, is so close that their coördination is not only justified but imperative.

The reviewer's criticisms can be limited to the first and the third chapters.

The first chapter contains a good deal of mysticism. The principle that race, language, and culture are to be coördinated is highly disputable. No matter whether it can be applied in considering "primitive" times, it is certain that the condition in the Near East, during historic as well as prehistoric times, is far from "primitive." The variety of ethnic names—I mention the Khatians, Lullubeans, Guteans, Kassites and Elamites; and how many may since have been entirely forgotten?—suggests a very complex situation which seems unduly simplified by the preponderance which Ungnad assigns to the Hurrites-Subareans. Furthermore we must

not draw hasty inferences from the ethnic situation of today concerning conditions in the IVth millennium B. C. Such a procedure involves a large margin of error, which in my opinion makes it practically valueless. And even if the existence of the "Steilschädel" in Mesopotamia of the IVth millennium is admitted, how can we know that this race was primarily Subarean? Why not pre-Subarean? If the type persisted in spite of all ethnic movement, it cannot give any indication whatsoever of an ethnic significance.

The "Buntkeramik" is treated as one of the achievements of the Hurrites-Subareans. Before it is used for historical conclusions, it should be properly located and dated. Ungnad takes for granted, first, that it covers all of Upper Mesopotamia; second, that it exists throughout the entire history of its mounds. Both these suppositions are wrong; in fairness to the author it must be added that this became clear only after the book had left his hand.

The investigations of M. E. L. Mallowan in the Khabur valley have furnished the invaluable information that the so-called Tell Halaf ware ceases to occur only a little south of the Tell Halaf and seems to have been introduced from a more northern region.<sup>2</sup> The valley of the lower Khabur is free from "Buntkeramik," and, we may add, none is known from the Euphrates valley.

The stratification of the Tell Halaf has not yet been systematically investigated. The evidence lacking must be supplemented from other sites. It is known now from Arpachiyah,<sup>3</sup> that the Tell Halaf ware precedes the Ubaid stage of the culture of Lower Mesopotamia; from Chagar Bazar,<sup>4</sup> a site not far from the Tell Halaf, that it is Chalcolithic and is followed at this site, at approximately 3000 B. C., by a very different type of monochrome pottery after the mound had been abandoned for a considerable length of time.

I may be relieved here from the duty of showing that the sculptures of Tell Halaf by no means certainly belong to the same archaeological layer as the painted pottery. I have discussed the matter at length elsewhere.<sup>5</sup>

The alleged unity of facts, then, vanishes with thorough examination. There remain dissociated features of the culture which may belong in quite different contexts.

<sup>2</sup> *Iraq* 3 (1936), 3 f.      <sup>3</sup> M. E. L. Mallowan, *Iraq* 2 (1936), 1-178.

<sup>4</sup> M. E. L. Mallowan, *Iraq* 3 (1936), 1-50. Cf. also L. W. Woolley on Carchemish in *Iraq* 1 (1934), 158-62.

<sup>5</sup> *Heftkriter, Churriter und Ausprer* (1936), 87 ff.

Ungnad locates Subartu in the Mesopotamian plains, and this location is essential for his argument. Nobody has denied this for the IIrd millennium. Ungnad's main effort is to establish it for the IIIrd millennium.<sup>8</sup> I cannot find that the evidence he offers is convincing. In fact, it consists of only one passage: the inscription of Naram-Sîn published by Sidney Smith in *Ur Excavations*, Texts I, *Royal Inscriptions* no. 374 (Ungnad, pp. 43 f. and 117 f.). It contains the statement that the king conquered the territory<sup>9</sup> of Subartu as far as the Cedar Forest, i. e. the Amanus (*ibid.* 275, 26). The *isakka* of Subartu are mentioned in the sequel beside the lords of the "Highlands."<sup>10</sup> It is true, the passage indicates a larger area of Subartu than had been previously assumed,<sup>11</sup> but the area indicated must be viewed in the light of the contemporaneous geography. The inscriptions of the Akkad dynasty make it perfectly clear that the Euphrates valley and Syria did not belong to Subartu, but were in the hands of the Amorrites. The "Highlands" of the above passage, according to Sargon,<sup>12</sup> include Mari, Yarmuti, and Ibla, and border on the Cedar Forest too. Subartu, then, must be pushed back to the north. It seems certain to me that there did not exist a Subartu which extended from Samarra and Kerkuk to Malatia and Carchemish when the Akkad dynasty conquered Syria and invaded Eastern Anatolia. The threat to the right flank of the conqueror would have been unbearable. Subartu, therefore, is to be located in the mountainous zone. It possibly included the basis of the Upper Tigris and circled around Assyria

<sup>8</sup> It is strange that the author fails to cite the previous discussions of the problem: B. Landsberger, *ZA NF* 1 (1924), 228 ff.; G. J. Gadd, *RA* 23 (1926), 68 ff.; Sidney Smith in *Ur Excavations*, Texts I (1928), p. 73; B. Landsberger, *OLZ*, 1931, 130.

<sup>9</sup> The usage of *kalam* in this context is exceptional; I refrain, however, from questioning its legitimacy.

<sup>10</sup> Some remarks on this inscription may be pertinent: for the ideogram contained in I 5 compare possibly Dnmal 444, 6B. Instead of *ka-ri-ia-ma*, the duplicate in Istanbul Asarlatika Müzeleri Negriyatı 12 (1934) pl. 5 no. 10 offers *ca-ri-ia-ma* (graphically very similar); a verbal form seems indispensable. In II 1 read *Tal-ja-tim*<sup>13</sup> and compare *Ta/What* (J. Lewy, *Die Kültepe-Texte der Sammlung Dehn*, 47).

<sup>11</sup> Landsberger has admitted this in *OLZ*, 1931, 130.

<sup>12</sup> *UMBS V* 34 + XV 41 col. VI. Cf. B. Landsberger, *ZA NF* 1 (1924), 228 ff.; Sidney Smith, *Ur Excavations*, Texts I (1928), p. 79 ff.; B. Malaler, *Untersuchungen zur alten Geschichte und Ethnographie Syriens und Palästinas* (1930), 6 ff.

in the north, just as did the Hurri countries of the Hittites and the Nairi countries of the Assyrians. With our present information, we cannot decide whether the region of Tell Halaf was Subarean or Amurritic.<sup>11</sup>

It is a fact which has always been recognized that there were Hurrites (Subareans) in the country east of the Tigris during the period of the 3rd dynasty of Ur. It cannot be seen how their presence, as Ungnad asserts, deals a final blow to his opponents who deny the existence of a Subartu within the Mesopotamian plains at this time. The emphasis laid upon this point might be justified, could Ungnad present a fair collection of Subarean names from Mari, Hama, Tell Halaf, or similar places. The evidence, as it stands, simply shows that after the downfall of the Akkad dynasty—probably in connection with the Gutean movement—Subareans entered the territory between 'Adēm and Upper Zab. It is safe to assume that they descended from the eastern mountains and the countries beyond, i. e. from the region where the Hammurabi period locates Subartu.

To sum up, I can find in the sources no support whatsoever for the assumption that Upper Mesopotamia south of the Tūr 'Abdīn was ever included in Subartu before the IIrd millennium.

The question of the terminology, i. e. whether Subartu is to be preferred to Hurri land, Subareans to Hurrites, seems to me of minor importance.<sup>12</sup> The latter, as the *ḫurru-ka umini* "Hurritic country" of the Tušratta letter shows, has the advantage of being a native term. The existence of a *ḫubar* (pronounce *ḫuwar*?) in the Sumerian column of geographical lists collecting ideograms of Subartu (Ungnad, p. 26) suggests that the term was not entirely unknown to Akkadians. The possibility must be kept in mind that *subar*, *sawar*, may be a mere dialectical variant of the same term; we know, however, too little of Hurritic and its dialects to make a special point of this.

Though Ungnad's main thesis, in my opinion, is to be rejected,

<sup>11</sup> Ungnad strangely underrates the significance of the Amurrites. It is in definite contradiction to the sources if he admits (p. 149, note 2) only sporadic and ephemeral Amurritic domination in Upper Mesopotamia.

<sup>12</sup> I believe with Ungnad in the virtual synonymy of the two terms. The designation "man from SU," in tablets of the 3rd dynasty of Ur (Ungnad, pp. 106, 137) must, however, be used more cautiously; only a few names of such people are Subarean.

the book has the merit of presenting the full evidence in the case. New material, it is to be hoped, will in the near future help to clarify further the situation. The publication of the Mari correspondence<sup>18</sup> also seems very promising in this respect. Excavations in a more northern site, best in Fecheriyah (Ungnad, p. 122) are most desirable. Baron von Oppenheim holds the concession; may he be enabled to use it!

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*A Hittite Glossary. Words of Known or Conjectured Meaning with Sumerian and Akkadian Words Occurring in Hittite Texts.* By EDGAR H. STURTEVANT. Second Edition. Philadelphia: LINGUISTIC SOCIETY OF AMERICA, University of Pennsylvania, 1936. 192 pages.

It is the great merit of E. H. Sturtevant to have provided all those interested in Hittite studies, particularly Indo-Europeanists, with an adequate and up-to-date introduction into Hittitology and its different aspects. His previous work to which the *Comparative Grammar* (1933) and the *Chrestomathy* (1935, in coöperation with G. Bechtel) bear witness, has now been rounded out by a new edition of the *Hittite Glossary*, which replaces the first edition of 1931.

Like its predecessor it is a full index to all contributions to Hittite lexicography. In the present stage of Hittitology its compilation was a necessary and useful undertaking. The pertinent literature has become so large and so widely scattered over numerous periodicals and publications that even those who work continuously in this field cannot longer do without such a tool. This time also etymological discussions have been listed, a procedure which will be welcomed by all linguists who wish to benefit by the progress in Hittitology. An etymological dictionary of the Hittite language cannot yet reasonably be expected. Some references to original texts which have not yet been treated as a whole are occasionally included.

The progress made in the five years which have elapsed since the first edition is evidenced by the fact that the book in its present

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<sup>18</sup> There is reason for the hope that they may provide us with a detailed picture of the political situation immediately before Hammurabi's conquests in the north. In the pertinent year-dates Mari, Ešnunna, and Subartu play an important part. Just these countries reappear in the specimen published by Thureau-Dangin in RA 33 (1936), 171 ff.

form contains more than twice the number of pages of the previous edition. This increase is only partly due to new items—as a matter of fact not a few items have been canceled as erroneous—but the author has paid much more attention to grammatical forms and morphological derivatives. It is now much easier for beginners to identify forms which depart from the usual pattern.

In a book like this it is quite easy for two different and incompatible readings or interpretations of identical passages to be recorded. This danger, as far as I can see, has successfully been avoided. I have found only two items which are included through such an error: *KIMMU* (p. 127); the passage really reads *É ka-ri-im-mi* (Sommer, who is credited with having established the existence of the word, has corrected himself in *Papankiri* p. 13). *IZZI*, gen. of *ISU* is suspicious because of its ungrammatical form (you expect *IZI*, i. e. *IŠI*); it seems to be parallel to the preceding entry.

The glossary employs a broad transcription of the original syllabic script. It is a necessary evil; any other procedure would cause confusion due to numerous variants. The reviewer has in conversation with the author objected to the use of the macron,<sup>1</sup> particularly in cases like *a-ar-ri*, *e-eš-si*; according to Akkadian usage the first vowel can hardly mean anything but an initial alif (glottal stop). In *a-a-ra* and *a-a-en-t*<sup>2</sup> an Akkadologist will believe that a *î* was implied (cf. Delitzsch, *Assyrische Grammatik*, sec. ed., § 20 ff.). Another objection was against spellings like *hwenhwas(s)ar*, *hwerzke/a-*; they still do not seem probable to me. Finally I do not believe that a *q* existed in the Hittite phonological system.<sup>3</sup> To my remarks in *Annalen des Museums*, p. 267 ff., should be added the observation that the older Akkadian syllabary, from which the Hittite syllabary is derived, cannot have possessed special signs for the Semitic emphatics. Wherever *qa* etc. do occur, they must, therefore, be read otherwise.

A few detailed remarks may be allowed. The reviewer wishes to make it clear that on all these errors and small corrections he blames himself explicitly; he overlooked them when the manuscript was before him.

<sup>1</sup> On the use of the macron Starckevant's introductory remark on p. 15 must be noticed.

<sup>2</sup> This is also Starckevant's opinion; nevertheless he uses *q* in quoting forms which are included between parentheses after the lemmata.

*esnas*, gen. of *eshar*: reference to Ehelolf, OLZ, 1927, 29 seems appropriate.

*haršan-* is the only possible reading, not only because of the "anaptyctic" *a* in forms like *har-aš-ša-na-ra* 2BoTU 23A II 51, but also because of *ha-ar-aš-ni* KBo II 19 rev. 13 and *ha-ar-ša-nu-aš* KUB V 7 rev. 30.

*Giššitananas*: add Götze, *Kultur*, 151, 153.

*istanussas* should be listed separately; it is the name of the deity belonging to the *istanas(n)-*.

*bakulupas*: with gen. *GIR-as* "ice" (KBo I 51 rev. 10).

*karimn-* has become suspicious, since Ehelolf has established *sinas* as the normal word for "god"; it possibly means "temple" the preceding *ê* "house" being a determinative.

*GIN*: I am very much inclined to return to this reading; it is definitely favored by the evidence of the contemporary Nuzi texts.

*but(t)as*: the nom. is *buts* (*bu-us-ra*) according to Bo 2002 I 8, 14 (unpublished).

*NIS DINGIR-LIM*: Akkad. *našum* (sic!) in this phrase means "live, recover," cf. Thureau-Dangin, RA 18, 154; 23, 26. In Arabic *na'asa* corresponds which explains the Akkad. *š*.

*SĀTU* is plural; the sing. is *ŠUTU*; cf. Thureau-Dangin, RA 29, 191 f.

*UŠTAHIZZU* is to be omitted; it is taken from an Akkadian text. *ZU*: cf. under *GIN*.

The printing is excellent; with Akkadian words a few wrong or missing accents have remained uncorrected. *SIBIR* is Sumerian, *ŠIBU* Akkadian and should be printed accordingly.

This excellent tool, for which we are deeply indebted to Sturtevant, will be used with gratitude by all workers in Hittite fields for many years to come. It is to be hoped that some day it may be replaced by a dictionary which will record the complete vocabulary of the published texts with the relevant quotations. But this is a project which is far in the future.

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*Studies in Dravidian Philology.* By K. RAMAKRISHNAIAH. Madras: UNIVERSITY OF MADRAS, 1935. Pp. x + 185. 3 sh.

The chief widely differing Dravidian tongues are Kanara, Tamil, Telugu, Tulu, in the south; Brāhui, Gōndi, Kui, Kurukh, in the north. Their interrelations are fairly well shown by their forms of demonstrative pronouns. Kanara has m. *asa* < \**asana* < \**aan* < \**asan*; f. *asaḥ* < \**asaḥ* < \**asāḥ* < \**asaḥ*; m. f. pl. *awaru* < \**awar* < \**aar* < \**akar*; n. *adu* < \**atu* < \**at* < \**aḍi*; n. pl. *awu* < \**awi*. Tamil has m. *avan*; f. *asal*; m. f. pl. *avar*; n. *atu*; n. pl. *avai* < \**avas*, an accusative used for the nominative. Tulu has m. *aje* representing \**ā* < \**aan* < \**asan* combined with the ordinary nominative-ending -e; f. *āḥu* < \**asaḥ*; m. f. pl. *āru* < \**akar*; n. *au* < \**aḍu* < \**aḍ* < \**aḍi*; n. pl. *aiḥuḥu* for \**ai* < \**awi*. Telugu has m. *vān-* < \**awan*, nom. *vādu* < \**avādu* < \**aanḥu* < \**asāḥḥu* < \**asanuḥḥu*; m. f. pl. *vāru*; n. *aḍi*, used also for the feminine; n. pl. *avi*. It has lost the f. \**asāḥ* < \**asaḥ*, nom. \**aanḥu* < \**asāḥḥu*, because of formal confusion with the masculine. Gōndi has m. *ōn-* < \**anon* < \**awan* < \**aan* < \**asan*, nom. *ōr* < \**awāḥu* < \**aanḥu* < \**asanuḥḥu*; m. pl. *ōr* < \**akar*; n. *aḍ* < \**aḍi*; n. pl. *au* < \**awi*. Kui has m. *am-* < \**aman*, nom. *andāu* < \**andru* < \**aanḥu* < \**asanuḥḥu*; m. pl. *āru* < \**akar*; n. *āri* < \**aḍi*; n. pl. *avi* < \**awi*. Kurukh has m. *ās-* < \**asan*; m. f. pl. *ār*; n. *ād*, used also for the feminine; n. pl. *ābrā*, perhaps from Aryan *sarva* (all). Brāhui demonstratives lack gender-forms.

From this outline it is clear that the eight tongues differ greatly in fonology and morphology, except that Kanara is rather closely connected with Tamil. Any serious historic study of Dravidian must take into account at least these eight tongues. Ramakrishnaiah's book does not give a single word-form from any one of the northern tongues. It therefore cannot be considered a really scientific work.

The first chapter deals with the general history of Dravidian and the Dravidians. The second and third chapters are made up of speculations on the source of various verb-suffixes and noun-suffixes. The fourth chapter discusses nouns and their relations to verbs.

In these four chapters I find little of importance. Some of the author's statements are surprising. In § 3 we read that the excavations at Mohenjo-daro and Harappa "go to prove . . . the identity of their civilisation with that of the Dravidians." This is news to me. § 13 seems decidedly unlucky. Here the author begins his

discussion of Dravidian speech with a statement that *var* (come), *tar* (bring), *ī* (give) and *veḷ* (shine) are primary roots. A knowledge of Kui would have shown him that the *r* of *var*- and *tar*- is not radical; it represents the personal-object suffix. Moreover *ta*-, with the variants *tai*- and *te*-, is a compound made up of *\*et* and a demonstrative *ai*-suffix; and *i*- was extracted from a compound of *\*et* and an *i*-suffix (*AJP* 59. 153). Further on we read of the pronominal roots *nā* (I) and *nā* (thou). These are not root-forms; they are alterations of basic *ēn* and *\*is*, lengthened from *en* and *\*is*, as I have shown in *Dravidian Developments*. On the same page we read: "Unlike the roots in Sanskrit or other Indo-European Languages . . . never found in actual use . . . Dravidian roots are used . . . as a form of imperative: *pō* — go, *tar* — bring, *paḍu* — fall." Universal negatives are dangerous. Allowing for fonologic changes, Latin *ī* (go) and *fer* (bring) and English *go* and *bring* are root-forms just as truly as *pō* and *tā*. I do not know where the author found such a word-form as *tar*; there is a present-stem *tar*-, replaced by *ta*- in the past and by *tā* in the imperative, in Kanara-Kēlan-Tamil. And *pō* is hardly a root-form; the root was rather *\*bō* or *\*bā*: the short vowels *o* and *a* are found in the imperative of Telugu, which seems to be the author's native tongue; *b* is found in Kurku *bō* (go). As Kolarian does not seem to show voicing-changes of initial occlusives, we may assume that *bā* represents Dravidian *\*bō* or *\*bā*. Telugu *paḍu* is not a root, nor even a stem; the stem is *paḍ*-, and the root perhaps *\*paṭ*.

In § 14 we read: "Strictly speaking, there is nothing like a case in these languages." Kanara has *ēna* < *\*jān* < *\*ēn* (I) with the genitive *ena*; Tamil has *jān* and *-ēn*, g. *ena*; Telugu has *ēna*, g. *nā* < *\*enā*; Kui has *ēna* < *\*ēn*, g. *nā* < *\*enā*; Gōndi has *anā* < *\*ēen*, g. *nā* < *\*enā*; Brāhūi has *i* < *\*ēn*, g. *kanā* < *\*enā* (*Dravidian Developments* § 45); Kurukh and Malto have *ēn*, with evidence of a former genitive *\*enā* or *\*ena*. We know, as well as anything historic can be known, that early Dravidian *ēn* had the genitive *ena* or *\*enā*. This genitive was just as truly a case as *tu* is a case of *tū* in Latin, or *its* a case of *it* in English. It is possible that *ena* *\*kutiras* (my horse) developed from *en* a *\*kutiras* (me that horse), but that does not alter the fact that *ena* is and long has been a genitive.

In chapter 2 the author discusses Caldwell's theorizing about the past-suffix *d* or *i*, and concludes that it represents *\*ūn* from *i*-

(give). Kanara *t-* has the past-stem *itt-*, which contains the *t*-suffix, so the explanation explains nothing. We simply know that early Dravidian had *t* or *d* as a suffix of the past and *tt* as a suffix of the perfect (*AJP* 50. 156): that is the limit of our knowledge on that point.

In chapter 3 the author assumes that \**avan* was the early form of Telugu *vān-* (that), and that it developed by a mysterious phonologic process thru \**avannu*, \**avandu*, \**vāṇḍu*, to the nominative *vāḍu*; and he declares that the *-ḍu* cannot properly be called a nominative-ending. A knowledge of Kurukh would have taught him that Tamil *avan* and Telugu *vān-* are derived from \**asan*. In nearly all varieties of early Dravidian there was a tendency to drop final consonants. The m. \**asan* and the f. \**asaḥ* became confused in a single form \**asa*, leading to the use of the neuter singular for the feminine in Kurukh-Malto. In Gōndi-Kui and Telugu an attempt was made to remedy the ambiguity of \**asa* by adding *uṣṭu* (being) to the stems: these tongues developed the nominatives \**asanuṣṭu* and \**asaḥuṣṭu*. But phonetic changes leveled these forms too: \**asanuṣṭu* became \**asaruṣṭu* and \**asaruṣṭu*; \**asaḥuṣṭu* changed thru \**asaṣṭu* to \**asaruṣṭu*, and the neuter singular had to be used for the feminine. The leveled form became m. \**awaruṣṭu*, \**awakuṣṭu*, \**awaru*, or in Gōndi; *awandū* in Kui; \**awāṇḍu*, \**vāṇḍu*, *vāḍu* in Telugu. The *-ḍu* of *vāḍu* is therefore a true nominative-ending. The author is likewise wrong in declaring that *-ḍu* is not a nominative-ending in the plural of Telugu nouns. In Telugu *strīḍu*, gen. *strīḍu*, dat. *strīḍaku*, acc. *strīḍanu*, voc. *strīḍārā* (women), *-ḍu* is a nominative-ending just as much as the *-ās* of *mulierās*, or rather more so, for *mulierās* is accusative and vocative also, while *strīḍu* is nominative alone. Of course the *-ḍu* is a plural-ending too, as the author says; but so is Latin *-ās*.

In chapter 4 the author assumes *kī*, with the variant *vi*, as a noun-suffix. The only example of the *kī*-suffix given is Kanara *kībī* [1] (ear). The Kanara word is *kivī*; *kībī* is hardly possible as a native word-form in Kanara. This *kībī* is put beside Tamil *kēḷi* (hearing) and *kēḷ* (hear). The Tamil word is *kēḷi* from *kēḷ*; it cannot be compared directly with *kivī*. Tamil has two words for "ear," *cevi* = Kanara *kivī*; and *katu*, with *ā* < *au* < *ou* < *eu*, corresponding to Kurukh *tebdā*. Perhaps these words are connected with *kēḷ* < \**kēḷ*; we might assume *kivī* < \**kēḷwis* and *tebdā* < \**kēḷwiḍa*. Tulu *kēḷi* seems to call for \**kēḷwis* < \**kēḷwiḍis*, which would do equally

well for Kui, as Kui has *r* for intervocalic *d*: *kriu* < \**kediū* < \**kediwis* < \**kṛiwiṣis*.

At the end of the book is an appendix of 46 pages, in which the author attempts to give the root-forms of a large number of words, chiefly verbs. But he does not show any true understanding of the basic principles of fonology. His first example is Kanara *aRi-*, Tamil *aRi-*, Telugu *aRug-* (know), with an assumed root *aRi*. He ought to know that Dravidian *R* (voiceless *r*) is a derivative of something else, namely *l* or *n* or *r* in contact with a voiceless sound. The fourth example is Kanara *uri-*, Tamil *eri-*, Telugu *eri-* (burn), with a highly unreasonable root *eri*. A possible southern basis might perhaps be \**ori*, but I think it is better to assume \**uri*, which by combination with Brāhmi *kaś-* < \**urs* < \**uris* becomes \**uris*. The crowning example of weak fonology seems to be *sā*, given as the root of Kanara *sā-*, Tamil *cā-*, Telugu *cā-* (die). Kanara *s* regularly represents an older *c*. Tamil has the past tense *cātan*, with *c* evidently older than the *ś* of the present tense; cp. *cāa* (my) beside *jān* < \**ān* < \**sa* (I). The northern *k*-forms show that *cā-* and *ce-* came from \**ke-*, or perhaps from \**ge-*. So *sā-*, instead of being the "probable primitive form," as the author calls it, is the form furthest away from the root.

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*Elements of Buddhist Iconography.* By ANANDA K. COOMARASWAMY. Cambridge: HARVARD UNIVERSITY PRESS, 1935. Pp. viii + 95, with 44 plates and explanation. \$3.50.

*La Sculpture de Bodhiḡaya.* Par ANANDA K. COOMARASWAMY. *Asiaticum* XVIII. Paris: LES ÉDITIONS D'ART ET D'HISTOIRE, 1935. Pp. iv + 74, with 1 plan and 70 plates. 200 francs.

Dr. Coomaraswamy's *Elements of Buddhist Iconography*, with his *Faṭsas* and numerous other publications, explains a number of motifs in Indian art and helps to transfer them from the category of ornamental to that of significant. Every one of these writings is a contribution of high importance and the result of prolonged research and acute analysis. In studying Buddhist iconography he justifiably seeks origins in pre-Buddhist sources and looks for enlightenment from contemporary non-Buddhist material. It is a

method which produced good results in his study of the origin of the Buddha image, and is indicated for the examination of all Buddhist types.

In his *Elements of Buddhist Iconography* he is trying to establish Vedic sources for the Buddha's Wisdom Tree, which he equates with the Vedic Tree of Life or World Tree; the Buddha's Lotus Seat, which he identifies with the World Lotus; the Buddhist Wheel of the Law, which he associates with the Vedic World Wheel; and the place of the Buddha's Lotus Throne, which he finds is the central point of the universe, the seat of Agni. The amount of material which he has collected on all these points, almost all of it new, is impressive; and he has established the point that there is a connection between the Buddhist symbolism and Vedic materials.

But there is an implication, which sometimes seems so strongly expressed as to exclude any other interpretation, that Buddhist symbology from the beginning has contained all the mystical content that can be found in the most advanced Vedic texts and later Buddhist literature. This point of view I find it difficult to accept. Take, for example, the Buddha's Wisdom Tree and the Vedic Tree of Life. I do not believe it is correct to state, or imply, that the Buddhist tree of enlightenment has always, and among all Buddhists, had the full significance that can be attached to the Vedic Tree of Life in the most esoteric (*brahmadhya*) type of Vedic thought. That type of Vedic thought is consciously recondite—as in RV. I. 164, which does not mean to be intelligible to any but the initiated. In finding origins and explanations for the Buddha's tree, I should attach considerably more importance than I find expressed in Dr. Coomaraswamy's book to the folkloric notions of tree-worship, which appears to go back to the Indus Valley seals. Indeed that sort of approach to the problem is followed in Dr. Coomaraswamy's *Bodhgayā*, as it was in connection with other problems in his *Yakṣas*. In a historical survey of the symbol and its use, the folk beliefs should probably come first; later the type so established was rationalized and the interpretation elaborated by mystic speculation. The situation with Indian plastic symbolism seems to me comparable with that of word treatment in the *Nirukta* and the *Mīmāṃsā*, which do not give correct etymology, that is, origin of the words, but rather the mystical association of words by sound for esoteric exegesis and application.

The appeal of early Buddhism seems to have been to the folk, at least down to the time of Asoka and the first Buddhist monuments, and it is to be presumed that to the folk whom Asoka's pious inscriptions and noble columns were to edify the meaning of the symbols was direct and obvious, not remotely speculative and mystical. Possibly even at Asoka's time advanced mystics were giving remote interpretations to the symbols, but Asoka's own simple words on his columns would not indicate that he was moved by their speculations and thought of them as the ideas he wanted to symbolize. The wheel at Sarnath meant the Law and its preaching, but it did not also mean everything else connoted by the wheel to late Vedic thought and esoteric Buddhism. Later these recondite ideas may have come to influence the plastic art, especially in the ornament of monuments belonging to mystical sects. Dr. Coomaraswamy's splendid study, therefore, seems to me open to some modification in the line of historical treatment, so as to work out the symbology from origins, probably simple and obvious, to more specialized application of varying sorts. That is to indicate that there were more varieties of Buddhism than an advanced mysticism, just as there were more varieties of mediaeval European Christianity than the kind represented by Eckhart, and that these other varieties held as important a place, perhaps even a more important place, in giving motifs to the plastic art.

His study of Bodhgayā is the sort of thing which we need literally in dozens for India, yet have only in a handful. It is a complete survey of the ancient site—its history, its architecture, its sculpture, its iconography. It even summarizes inscriptional material, although Dr. Coomaraswamy states that this topic is beyond his full control and the book's scope. It covers, on the topics treated, the literature to date of publication, advances well substantiated new explanations of some details, and points out what is still to be explained at the site. There are excellent illustrations of all the remains. It is a scholar's guide and source book.

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*Indian Pictorial Art as developed in Book-Illustrations.* By HIRANANDA SASTRI. Gaekwad's Archaeological Series, No. 1. Baroda: BARODA STATE PRESS, 1936. Pp. iv + 24, with 19 plates. 13 as.

The Gaekwad of Baroda is justly honored as one of the most able rulers of modern India, whose state shows the result of his interest in education, public health, and his people's welfare in general, and whose outside activities show his concern with human questions on a large scale. A man of great appreciation for things of the spirit as well as those of the flesh, he has established a library and research publication department at Baroda, which has now attained an unexcelled position in Indic scholarship for its Gaekwad's Oriental Series. He has recently established a department of archaeology for his state, and appointed as its director Dr. Hirananda Sastri, retired Government Epigraphist for India. In view of the many important archaeological monuments in the Baroda state and the possibility that sites existing there will on excavation prove important, this new venture is one of significance to Indic scholarship.

In the first volume of the Gaekwad's Archaeological Series the director sets out to refute the alleged opinion of other scholars that India did not have a pictorial art of book-illustration. In recent years, certainly, no one has held this opinion, and Dr. Sastri's thesis seems a man of straw hardly worth while for anyone to attack.

In his discussion he adduces some interesting diagrams from manuscripts, which come under the head of book-illustration, although hardly of pictorial art, and are valuable in showing the early history of a phase of book-illustration, at least allied to pictorial art. He also cites some literary passages on *mudrās* (hand-poses) of the sort illustrated in some manuscripts.

The main part of his booklet deals with painting from Gujarat. Most of his material is of small importance, as adding little to known and published types of illustration, but he shows two illustrated folios of a manuscript of Hemacandra's *Siddhahemalaghuṣṭi*, not dated, but thought by Dr. Sastri to be of the 12th century. It may be; but the paintings, as far as I can see from the very small and poor reproductions, are much like dated illustrations I know in 13th century manuscripts.

He also, strangely, advances an illustrated paper manuscript of the Jaina *Kalpaśūtra* as a work of Vikrama Samvat 1125 (A. D. 1068).



He bases his opinion on the colophon, which reads *vīkramaśamvat 1125 dipamālādīne saiddhāntikāśīromanjanenwicandraśrīpā suvrat-kāśe śikṣāpitam*. Since no such early paper manuscript is known for India, this would be a startling discovery; so too its illustrations would be the earliest known miniature paintings from western India. The date and Dr. Sastri's conclusions are fallacious. It is to be noted that the colophon does not claim to make any statements about the time when the manuscript was copied; it merely states that "On Diwali day, in the year Vikrama Samvat 1125, the śrī Nemicaṇḍra, crest-jewel of the Scripture, fixed [the text] in his heart (i. e., committed it to memory?)." The handwriting of the colophon is obviously different from that of the rest of the work, as appears in Dr. Sastri's reproductions; notice the difference in making *d* (especially the "tail"), *v* (squared in the colophon than elsewhere), *m* (much larger opening in the colophon), *s*, *p*, *k*. The final sentence is a later addition; how the erroneous date got into it I do not venture, or need, to guess. The very appearance of the manuscript is against this manuscript being early, even for paper, which became common for books in western India around 1400. In the earliest known reliably dated paper manuscripts the proportion of dimensions is closer to that of the dimensions of the palm folios. Here the proportions are those of positively dated manuscripts of the early 16th century. The folios of this manuscript compare almost exactly with those of manuscript Hc in my *Miniature Paintings of the Jaina Kalpasūtra* (see p. 3, and Fig. 15), which is dated Vikrama Samvat 1577 (A. D. 1520). Similarly, Dr. Sastri's manuscript has the same number of lines to the page, the same large writing, the same style of festooned lozenge-ornaments to mark the (old) string-hole spot and page-number spots. The paintings are placed in the same way, and the whole appearance of the page is similar. Many other 16th century manuscripts of the same style are known; and the 16th century, or possibly late 15th century, is the earliest date that can by any means whatever be assigned to his manuscript. The style of painting is also similar. Earlier dated paper manuscripts appear in my *Kalpasūtra*: see B 17. 2277, Hd, Hf, M 18. 104. For manuscripts, all palm, of a period only shortly after that to which he assigns his paper manuscript, see in my *Kalaka*, plates 1, 2, and his own plate 10.

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*La Peinture Iranienne, sous les derniers Abbasides et les Il-Khāns.*

Par IVAN STCHOUKINE. Bruges: IMPRIMERIE STE. CATHERINE, 1936. 188 pp., XLVI plates.

The History of Persian Painting, as Dr. Stchoukine properly lays down in his preface, now finds itself facing the delicate study of problems of detail. To this study he devotes his witty, lucid, and judicious book. After an admirable résumé of the theories of his predecessors in the field, upon whose militant asseverations his courtesy is not the least pointed of his comments, he assembles a much-needed corpus of manuscripts and of historical references, and embarks on a detailed analysis of the paintings in his list.

This analysis seems to establish his general thesis. Abbasid painting, in spite of heterogeneous influences, is predominantly Iranian: far-Eastern influences which are in full play at the beginning of the fourteenth century gradually give way to the culmer line and gay polychromy of the old Iranian style. So far (and so far, it is to be observed, he follows closely the scheme indicated by M. de Locey),<sup>1</sup> and in his discrimination of three successive Mesopotamian styles, primitive, realistic, and stylised, his analysis supports the validity of the categories he employs.

But minuter attention suggests that an important factor in the formulation of the mature Persian style is neglected by Dr. Stchoukine. Although he mentions Ibn Bawwab the calligrapher as a fresco-artist, and takes al-Wasiti, the calligrapher-painter of the Schefer Hariri, as the typical master of his second Mesopotamian period, he barely pays the customary lip-service to calligraphy as a decisive factor in the evolution of Persian painting. From this omission follows his mistaken attribution of the characteristic fine line to far-Eastern sources. The turbans and drapery of the Hariri Schefer,<sup>2</sup> and, still more definitely, such drawings as the British Museum sketch (repr. Glück & Diez, *Kunst des Islam* 503b) display a line as fine as any in the Mongol period. Again, the asymmetrical compositions of that period derived from older Iranian formulae (cf. the "Death of Moses," in the Jāmi' al-Tawarikh,<sup>3</sup>

<sup>1</sup> "La Peinture Musulmane. L'Ecole de Bagdad" (*GRA* 1933) and "L'Ecole de Tabriz" (*RAA* 1936).

<sup>2</sup> A list of reproductions is given by Stchoukine, p. 71, No 3.

<sup>3</sup> Blochet, *Musliman Painting*, Pl. XLIX.

with "Erasistratus and his Famulus" in the Sarre collection, Berlin).<sup>4</sup>

Dr. Stehoukine neglects ceramic painting, an invaluable index of the pictorial art destroyed, in its more destructible vehicles, by the Mongol invasion. The figured pottery, of Rayy, Kashān, and Sāva, represents a school of painting, flourishing in central Persia in the twelfth and thirteenth centuries, from which many of the most critical characteristics of Mongol and Timurid art derive. Here is a lively naturalism expressed in purely calligraphic line. Here, long before the Mongol invasion, is a round, linear-featured face of "Mongol" type which vitiates Dr. Stehoukine's analysis (p. 138), and offers a sharp contrast with the heavy-featured face of Raqqa ceramics, so close to the late Mesopotamian illustrations. That this linear style was not confined to ceramics the existence of some frescoes from the Kevorkian collection<sup>5</sup> offers proof.

A just formulation of the component elements in Mongol painting is impossible without the collation of more material than Dr. Stehoukine offers. But even if his categories were too hastily chosen, and his order imposed rather than deduced, nevertheless his book strikes an attitude of careful observation which future writers will probably be wise to imitate. His studies pick out Byzantine, Chinese, or Sasanian features, all perhaps really external to the body of an art which was organic and, from Saljuq times at least, continuous; but the native features must be traced with the same conscientious care as Dr. Stehoukine's, by those who wish to establish its claim to originality on a scientific basis.

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<sup>4</sup> Kuhnelt, *Islamische Miniaturmalerei*, Pl. 5.

<sup>5</sup> One of these is in the possession of Fogg Art Museum, Harvard College. Others are now (Jan. 1937) on exhibition there.

*Saṃdhinirmocanasūtra, l'explication des mystères.* Texte tibétain édité et traduit par ÉTIENNE LAMOTTE. Recueil de travaux publiés par les membres des CONFÉRENCES D'HISTOIRE ET DE PHILOLOGIE, 2<sup>e</sup> Série, 34<sup>e</sup> Fascicule, Louvain: 1935. Pp. 278. Price 30 belgas; 75 French francs.

The *Saṃdhinirmocanasūtra*<sup>1</sup> is another important Buddhist text whose Sanskrit original has been lost. It has been preserved only in translation: one Tibetan and five Chinese,<sup>2</sup> of which two are incomplete. In addition there are four commentaries in Tibetan, one of which is a translation of Yüan-ts'ü's 圓珠 Chinese commentary. Dr. Lamotte has edited<sup>3</sup> the Tibetan text and translated it with line and page references to Hsüan-tsang's translation. Of the commentaries only the one by Asaṅga has been used.

This text is a catechism of Mahāyāna Buddhist doctrine, valid, according to the author, for the first centuries of the Christian era. It would seem to have been compiled gradually of different short texts during the second century and to have reached final form by the beginning of the third. It is slightly later than the *Daśabhūmika* and the *Kāśyapaparivarta*, and contemporary with the oldest parts of the *Laṅkāvatāra*. Dr. Lamotte has made a genuine contribution to our studies by making easily available a text that should be very useful as an introduction to Mahāyāna Buddhism. Both the catechistic style of the text itself and the numerous commentaries and translations provide excellent material for a first seminar in Buddhism.

The present volume is an excellent piece of work, and the reviewer is glad to recommend it; he hopes that it will be consulted by all interested in Buddhism, and, particularly, that it will receive the extensive use in classes that it deserves. It exhibits the high degree of care and thought to be expected of a man who has had the good fortune to work for ten years under the tutelage of such a master as Professor Louis de La Vallée Poussin.

The edition of this text should provoke among Sanskritists a

<sup>1</sup> This form is guaranteed by the Tibetan transliteration. The Tibetan title is *Byphags-pa Dgongs-pa Nes-par byrel-pa Theg-pa Chen-pohi Mdo*.

<sup>2</sup> Nanjio 154-155, 246, 247. Lamotte says nothing about the lost Chinese commentaries; cf. *Buddhōdsaijī* 1028C.

<sup>3</sup> The transcription of Das's dictionary is followed with the one exception that *sh* is used for *śh*.

series of articles in the attempt to clarify the semantics of *saṃdhi* and the verb *saṃdhi*. Wassiljew<sup>4</sup> has translated the title of our sūtra as "Erläuterung des Willens, wörtlich Lösung der Verbindung." Bendall and de La Vallée Poussin<sup>5</sup> would translate as "mystery" or "secret." Lamotte recalls that the Tibetan rendering of the word, *dgoṃs-pa*, "purpose" or "intend," is most frequently used to translate *abhiprāya*, "intention." He then assumes that *dgoṃs-pa* and *saṃdhi* are possibly abbreviations for *ldem-por dgoṃs-pa* and *abhisamdhī*, "intentional teaching," and suggests the translation "hidden intention." Paramārtha's Chinese translation renders this difficult word literally by *chieh* 節, "joint." The two partial translations attributed to Guṇabhadra employ the term *hsiang-hsü* 相續 which is the equivalent of either *saṃdhi* or *saṃtāna*. On the other hand, both Bodhiruci and Hsüan-tsang translate the word by *shēn mī* 深密, "profound secret."<sup>6</sup>

After having admitted on p. 13 that the title of his text means literally "Sūtra détachant les nœuds," Lamotte (cf. especially p. 20-21) misunderstands the title of Paramārtha's translation. It is impossible for me to believe that *chieh* can represent here anything other than a translation of *saṃdhi* whereas Lamotte would take the word with the preceding *chieh* 解 as the translation of *nirmocana*. He would have done better to follow Nanjio.

It is surprising, to say the least, that in the list of sources no mention is made of Yüan-ts'ŭ's *Chieh-shēn-mī-ching Su* 解深密經疏 published in *Kyōto, Suppl.*, 1, 34, 4, 5 and 1, 35, 1, while the Tibetan translation of this commentary is noted. On p. 17 Lamotte asks a question which I find incomprehensible: "Pourquoi existe-t-il un commentaire de Ven-tshig sur le chap. IX seulement?" "Ven-tshig," as Lamotte well knows (cf. p. 11), is the Tibetan transliteration of Yüan-ts'ŭ (sixth century Yüan-ch'ien), who, according to a brief notice in *Sung Kao-seng-chuan* 4 (Taishō 50, 727B), was a student in the time of Hsüan-tsang. His commentary

<sup>4</sup> *Der Buddhismus*, St. Petersburg, 1860, p. 162.

<sup>5</sup> *Musson*, 1909.

<sup>6</sup> The translation of the title of our sūtra given in Sakaki's *Mohyōgū*, 1359, *Kuanyō-kai Chien-shih Ching* 廣開真實經 (something like "Revealing-truth-sūtra"), may be passed over as a late, euphemistic, vague rendering.

<sup>7</sup> Cf. also *Liu-hsueh-sung Chuan* 22 六學僧傳, *Kyōto, Supplement*, 3, 3, 5, 420aA.

was originally on the ten chapters of the *Saṃdhī*<sup>2</sup>, but the text published in two and one half volumes of the Kyōto tripiṭaka lacks the last chapter, the commentary to chapter 10 of the *Saṃdhī*<sup>3</sup>. Since nothing is said to the contrary, it is reasonable to suppose that Yüan-ts'ê's work has been preserved intact in the Tibetan translation, for it covers three volumes in the Tanjur. Therefore, Lamotte's question is to be deleted.

Given the difficulty of access to editions of the Kanjur and Tanjur, it may be helpful to students if the reviewer appends to this notice the variations he noted between the text as edited by Lamotte and the Narthang edition of the Kanjur. For this purpose the following pages of Lamotte's edition have been collated: 31-45, 88-97, 149-166. 32, 6 f. (*rom*) *b* (*ottom*), thams cad kye ye; 5 f. b., med pa / dam; 3 f. b., dan dbuñ med. 33, 5, legs par bsam pa. 35, 10 f. b., rname kyi lhphegs. 36, 10, mtheñ has brjod du; 20, bai dūrya dan; 22, tshogs rnam par bstan na /; 30, sñam du sems gin /. 37, 6, bañ bañi tshogs. 38, 18, byas dan / hdu. 39, 6 f. b., so—sñam; 4 f. b., gsol pa dāñ. 41, 2, pa ni nañ; 13, blag gi yodis. 42, 8, dan / don; 13, dan / don dam. 43, 1, gsol pa dan; 17 f. b., kyi mshan ma. 44, 1, par rñogs par htshuñ; 16 f. b., dan / don dam. 45, 13 f. b., dan / don dam; 3 f. b., med pañ [tsam. 86, 15 f. b., brñan dan; 7 f. b., dan / dgoñs pa. 89, 12, sems dpus legs. 90, 4 f. b., ma yin [ma yin]. 91, 4, cea pa ni dmigs; 10, yin pa de de ltar. 92, 15 f. b., pa / roam. 93, 5, po pa dan /; 10 f. b., rjes su brtan pa; last line, rjes su hbañ ba dbañ. 94, 3 f. b., gxhol pa / byañ; 7 f. b., / gnes gyur pa; 6 f. b., ba la hbañ pa [43<sup>b</sup>] /. 96, 7 f. b., yin pa de ni /; 4 f. b., snañ ba hol byi. 97, 8, med ciñ dpyed. 149, 3, chos sku zhes. 150, 11, dkañ ba spyed pa; 12, tu ston po dan. 152, 5, la legs par. 155, 4, rkyen gzhi; 12, pa dan / rkyen. 156, 1, mi brtag pa; 4, pa de ni mñon; 7, riag par rigs; 3 f. b., ba dan / de. 158, 7 f. b., / de [la gzhan; 6 f. b., pa bstan pañi; 5 f. b., ſid kyi yodis. 160, 7 f. b., / zaño byed de /. 164, 3, rigs giñ sñ; 13, lus phun sum tshogs. 165, 8 f. b., dpal hdi de. 166, 3, dgoñs pa ñes.

*Tibetan Literary Texts and Documents concerning Chinese Turkestan.* Selected and translated by F. W. THOMAS. Part I: Literary Texts. Oriental Translation Fund, New Series, Vol. 32. London, 1935. Pp. x + 323.

Professor Thomas has collected into this volume translations of, with one exception, all the Tibetan literary references to Chinese Turkestan. The Tibetan materials secured by Sir Aurel Stein will be discussed in Part II of the publication which will form a second volume. While all of us are grateful for this genuine contribution, it is most regrettable that there seems to be no intention of printing the texts themselves. So great, however, are the difficulties attendant upon any attempt to obtain Tibetan books, that no Tibetanist should feel his job complete until he has published the text of any translation. Such is the worthy example set by Dr. E. Lamotte in his *Saṃdhi-nirmocana-sūtra*, and it is earnestly hoped that Professor Thomas will agree to this principle in practice.

The present volume contains careful, annotated translations with introductions of the following material: 1, *Gośrūga-vyākaraṇa* or *Bi-glān-ru Lun-bstan-pa* (The Prophecy of Gośrūga), *Narthang Kanjur*, mDo 30(a).336b<sup>2</sup>-354b<sup>4</sup>; 2, *Arhat-Saṃghavaradhana-vyākaraṇa* or *dGra-bcom-pa dGe-hdun-ḥ-pheḥ-gyi Lun-bstan-pa* (The Prophecy of the Arhat Saṃghavaradhana), *Narthang Tanjur*, mDo 94(āc).412-420; 3a, *Li-hi-gul-gyi Lun-bstan-pa* (The Prophecy of the Li Country), *Narthang Tanjur*, mDo 94(āc).420b<sup>1</sup>-424a; 3b, *Li-gul-gyi Lo-rgyas* (The Annals of the Li Country), *op. cit.* 424b<sup>1</sup>-444a<sup>4</sup>; 4, *Dri-ma-med-paḥi-kod-kyis Shus-pa* (The inquiry of Vimalaprabhā), *Narthang Kanjur*, mDo 13(pa).339-418; 5a, *Śīlavīropaniṣantṛaya* (Invitation of Elders), *Narthang Tanjur*, mDo 94.403b<sup>1</sup>-412b<sup>4</sup>; 5b, *The Sandal Image of Buddha*, *Narthang Tanjur*, rGyud 85(ru).142a<sup>1</sup>-144a<sup>6</sup>; 5c, Selections from the *Padma-bkaḥi Than-gig* (Writing of the Pronouncements of Padma[sarabhava]), which circulates in a separate xylograph volume and is not to be found in either the *Kanjur* or the *Tanjur*; 5d, Selections from the *U-rgyan-guru-Padma-ḥbyun-gnas-kyi sKyes-rabs-rnam-thar* (Account of the Lives of the Guru Padmasarabhava of Udyāna), which also circulates only in separate editions; 5e, Selections from the *rGyal-rabs-gsal-baḥi Me-loḥ* (Mirror Exhibiting the Royal Lineage), which circulates only in a separate edition; 5f, A selection beginning at p. 50a<sup>2</sup> from the *bKaḥ-ḥgyur-rin-po-cheḥi gSṅaḥ-par*



*Srid-gsum rGyan-gcig rDau-bphrul-gin-rtshi dKar-chag No-mtshar-bbed-pa-rgya-mtshohi lDe-mig* (Key to the Marvellous Ocean of Compilations, An index to the Miraculous Vehicle, the Single Ornament of the Triple World, the Printed Word of the Precious Kanjur); 6, *Li-yul-chos-kyi Lo-rgyas* (The Religious Annals of the Li Country), from a Tun-huang MS now catalogued at the French Bibliothèque Nationale as *Fonds Pelliot tibétain No. 254*.

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Yuen Ming Yuen, *L'Oeuvre Architecturale des Anciens Jésuites au XVIII<sup>e</sup> Siècle*, by MAURICE ADAM. Pei-p'ing: IMPRIMERIE DES LAZARISTES, 1936. Pp. xiv, 44. Profusely illustrated.

Yuen Ming Yuen, or Yüan Ming Yüan, is the name of the most famous suburban palace of the Manchu emperors of China. It was one of a large group of imperial villas lying in a well-watered and historic region extending from five to ten miles northwest of old Peking close to the present sites of Yenching and Tsinghua universities. The climax of the palace building in this region, as well as the climax of the splendor and prestige of the Manchu dynasty, came during the reign of Emperor Ch'ien Lung (or K'ien-long), 1735-96, and at this time Jesuit missionaries at his court were employed to construct a set of European palaces in a small park immediately adjacent to the walls of the Yüan Ming Yüan.

Maurice Adam, who died in 1932 at the age of forty-three, had collected considerable material which he intended to use in a history of the imperial palaces of China. That part of his work which dealt with the Jesuit missionaries and the European buildings at the Yüan Ming Yüan was so nearly complete at the time of the author's death that his friends arranged to have that part edited and published as a memorial to him. The title of the work as published should not be understood as indicating a complete treatment of the whole Yüan Ming Yüan.

In his descriptions of the European buildings M. Adam gives us some strikingly new information, e. g., on the festival of the Yellow Flowers in the Labyrinth; the use of two of the buildings

by the famous Hsiang Fei (Ch'ien Lung's Mohammedan concubine); and the painting of the panorama of a foreign town in western perspective on brick walls to represent the city of Aksou (home of Hsiang Fei), instead of a European town as had been supposed previously. This information would be more valuable to students of the history of this palace if the editors had supplied references to Adam's authorities for these interesting statements.

The work consists in part of long quotations from the sources. One of these is the famous letter from Father Attiret in 1743 describing the Yüan Ming Yüan before the construction of the foreign buildings (pp. 3-12), while others describe the making of the fountains and the copper engravings; the chapter on the burning of the Yüan Ming Yüan in 1860 consists almost entirely of quotations from several French writers blaming the English alone for the destruction of the palace, but making no reference to the English writers who usually complain that the looting of the palace was begun by the French before the English arrived.

Many of the best drawings and maps were contributed by M. Kin Hsiün (Chin Hsün 金勳); some of these are new drawings and some copies of original architect's plans for the builders, handed down in M. Kin's family from the time when his ancestors were contractors in charge of the construction of certain parts of the European buildings. M. Adam has made a real contribution in his discovery of this material in Chin Hsün's family archives.

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*Les Fêtes Saisonnières au Japon.* (Province de Mikawa). Etude descriptive et sociologique. Par N. MATSUDAIRA. Paris: G.-P. MAISONNEUVE, 1936.

This study is, with the addition of an introduction and a brief conclusion, divided into three parts of unequal length. The first of these (pp. 13-74), is devoted to a description of festivals celebrated yearly by the inhabitants of certain villages in the province of Mikawa. These festivals are: (1) the *Hanamatsouri* or feast of flowers (Matsudaira's transliteration of Japanese words, it may

be noted, is that employed by the French, not that of the *Romajikmai*; (2) the closely related *Mikagoura*, which may well be the ancient form of the *hanamatsouri*; (3) the *Kagoura*, which has not been celebrated since 1855 and which, unlike other similar festivals, came but once in seven years; and (4) the *Dongakou*, a fête that is observed not, like the others in November, but in January. A brief note on summer festivals, held in July, and another on spring and autumn festivals, held in May and mid-October, complete this section. Matsudaïra gives a detailed description of the preparations for these matsouri, of the participants, and of the dances, with numerous quotations—some of considerable length—from the dialogue and chorus. The second part (pp. 77-132), is a consideration of the beliefs underlying and indicated by the festivals: the spirit (*ama* or *misaki*) which "inhabits the body that it animates and gives to it sensibility, intelligence, and the gift of dancing"; the village deity in whose honor the villagers hold their festivals; the divinities who are summoned to be present; the half-gods (e. g., Hino, king of fire, and Mizuno, queen of water) who figure in the performances. The third part of the work (pp. 135-168), treats of the position of the individual in the village economy and the organization of the village under religious and secular leaders as shown in these fêtes.

From occasional mention in the text, as well as from the introduction and conclusion, it is clear that this was written primarily to supplement Hayakawa's work on the *Hanamatsouri* (Tokyo, 1930), a work of great value, in Matsudaïra's judgment, for its encyclopedic wealth of information on the dances and their history, but one which neglected matters of equal or greater value from the sociological point of view. The reviewer has not seen Hayakawa's book, but it seems a reasonable supposition that the first section of *Les Fêtes Saisonnières* is, in some measure, a briefer parallel account, the purpose of which is to lay a foundation on which the author may erect the superstructure, his analysis of the beliefs indicated by the festivals and the organization of the village. The conclusions that he reaches may be briefly summarized. (1) The development or growth of the individual soul from the moment when it first animates the body to the time when it leaves this earth is affected by ceremonies performed in connection with these festivals, and the relation between the growth of the soul and the

ceremonies is set forth in the dances and the dialogues. (2) Each of the villages studied has several Buddhist and Shinto temples; in general, one of the Shinto temples is the dwelling place of the village deity. In antiquity the deity was thought to inhabit some sacred tree, at the foot of which the villagers held their fetes. Later, when the tree died or was cut down, the wood was made into a shrine, and the festivities naturally enough were continued before the shrine or, yet later, about the temple that was constructed near it. (3) All the gods of the universe are invoked to take part in a reunion, by magic ceremonies, offerings, music, and dances; so the festivals are performances not merely in honor of the local deity but calculated to win the favor of an entire pantheon. (4) The organization and life of the villages is of such sort that individualism is quite inconceivable; the family, not the individual, is the unit.

In all this, there does not seem to be anything strikingly original. Dr. Matsudaira's study is well documented; he justly calls attention to the wealth of Japanese folklore, justly points out that it is a field that still offers opportunities for research, gives us a clear and well-reasoned account of certain ancient popular festivals and their background. It may be ungracious to find fault with him for doing no more, for evidently that is all he intended to do. The criticism that the present writer would offer it this: Matsudaira, like some other Japanese scholars dealing with the myths and folklore of Japan, gives no hint nor indication that Japanese mythological concepts are by no means unique, that similarities and parallels can readily be found. In other words, such a study as this would be more valuable, both in its descriptions and in its sociological analysis, were it so widened in scope as to find room for a comparative treatment of the matters with which it deals. Yet this criticism in no wise implies that Dr. Matsudaira has failed to perform thoroughly and satisfactorily the task which he set himself and which was undoubtedly worth doing.

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*Une Poétesse Japonaise au XVIII<sup>e</sup> siècle, Kaga No Tchiyo-Jo.* Par GILBERTE HLA-DORGE. Paris: G.-P. MAISONNEUVE, 1936.

This is an unusually interesting work on Japanese poetry. A decade ago, or thereabouts, Mme. Hla-Dorge, then Mlle. Dorge, was working at the Sorbonne on the history of Japanese civilization and at the École des Langues Orientales on the Japanese language; in writing this doctoral thesis she has shown the advantage of possessing not alone mature scholarship but also a sympathetic insight into the thought and expression of the Japanese poets of the eighteenth century. So far as Japanese poetry is concerned, she points out, quite justly, that while the Japanese are in the main a nation of imitators (with, it should be added, a remarkable facility at assimilation) their poetry is *sui generis*; it may, and occasionally it does, contain echoes of Chinese thought, but the form of Japanese poetry is entirely other than that of the Chinese. The simplicity, brevity, and directness which are dominant characteristics of Japanese poetry are particularly clear in the form of versification called *haikai*, a poem of seventeen syllables. Of the poets and poetesses who excelled in this form none is more famous among the Japanese than Tchiyo (Chiyo would be the normal transliteration, but Dr. Hla-Dorge explains in her introduction that she writes with French readers in mind), some five hundred of whose poems are given, with translation, in this volume.

Kaga-no-Tchiyo was born in 1703 in a village within a few miles of Kanazawa. Her father was a maker of *kokemono*, and as a small child she played in the workshop where he was employed, saw there the works of art and the specimens of calligraphy which it was his task to mount. When she was nineteen years of age she was married to a retainer of the daimyo of Kaga; when he died ten years later she became a Buddhist religious, a consecrated widow living not in a convent but in her own house. In the year 1776 she died.

Such is the slender biographical framework about which this study is constructed. What Dr. Hla-Dorge has given us is a literary biography, with an introductory chapter on Japanese poetry and another on women of letters who composed *haikai*. Not till page 72 does Tchiyo step upon the stage. From there on a somewhat discursive narrative traces Tchiyo's poetical career and gives a vast deal of information regarding the ideas and ideals of her time.

For those who wish to penetrate beneath the surface in an endeavor to understand the spirit of old Japan in its fairer aspects, this book can be safely, and highly, recommended.

ALFRED H. SWEET.

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*Le Vieux Japon: "La Route de la Mer orientale."* By MICHEL RIBAUD. Voyage du Jeune Stanislas au Japon, ou Essais sur la Civilisation japonaise—Histoire, Religion, Philosophie, Littérature & Beaux-arts. Volume 11. Paris: LIBRAIRIE D'AMÉRIQUE ET D'ORIENT, 1935. Pp. 256.

The first volume of the series "La Traversée, Tokio" was an introduction to the cultural background of the entire Far east; hence the author's suggested title, "Discours sur la Civilisation asiatique," is very apt. The present volume, the second in the series of four volumes, deals with the evolution of the island empire and traces its development from the earliest known date to the beginning of the Tokugawa regime (1603). Leaving the modernized city of Tokyo, the caravan journeys southward on the Tokaido highway, which for centuries was the main artery. Various post-stations such as Okitsu, Hamamatsu, and Nagoya provide excellent background for the legends and myths such as the story of the "Feather Robe" of the *No* play and the odyssey of Urashima. The pilgrimage to Ise opens up the history of Shintoism; the ancient city of Nara gives an occasion to narrate the introduction of Buddhism and Chinese culture, the new impulse to creative arts, and the subsequent rise of the native language and culture.

M. Ribaud incorporates an amazing amount of information in his small volume, and the journey is made exciting not only with history and ancient folk-lore, but also with keen observation of modern every day life. A charming dinner party, a tour through a modern porcelain factory, and the life and belief of young Japanese are skilfully woven in the narrative. Moreover, his discourse on civilization fits in well with the geographical features. His chapters on the early feudal regime and the spread of Christianity are slightly out of place, and if Mr. Ribaud is rather dogmatic on such controversial subjects as ethnology and anthropology, it is due to the nature of the book. On the whole the book is extremely informative and stimulating.

*Les Notes du Chevet de Sei Shônagon, dame d'Honneur au Palais de Kyôto.* (Traduction in extenso de l'ancien texte japonais).

By ANDRÉ BEUFARD. Paris: LIBRAIRIE ORIENTALE ET AMÉRICAINE, 1934. Pp. xxii, 329.

It seems that every Western scholar who went into Japanese literature tried his hand at the *Makura no Sôshi* of Sei Shônagon, but "because the original was dull, unintelligible, or so packed with allusion that it required an impracticable amount of commentary," only a quarter has been translated. Now for the first time M. Beufard translates it in its entirety with copious notes. He followed the excellent commentary of Kaneko Motomi, first published in 1924, and also Mizoguchi Hakuyô's translation into modern Japanese, issued in 1927. The parts which the Western scholars had thought dull and repetitive in the present translation become thoroughly alive with historical interest, and his notes are invaluable for the students of ancient Japanese literature.

The *Makura no Sôshi* is a plain record of facts, and consists partly of reminiscences and partly of entries in diary-form. The book is not arranged chronologically, but under a series of headings, though often this scheme breaks down and the sequence becomes entirely arbitrary. The authoress, Sei Shônagon was born in 966 or 967, the daughter of Kiyohara no Motosuke, whose ancestors distinguished themselves for their devotion to learning and literature. Shônagon entered the service of the Empress Sadako at the age of twenty-four or twenty-five and remained in the service for ten years till the Empress died. It is with these years, from 991 to 1000, that the *Makura no Sôshi* deals.

The present translation is done with the utmost accuracy and M. Beufard has not only caught the author's unique prose style but also penetrated the aesthetic and literary civilization of the time. On the more debatable passages in the text, I refer the students to the more recent edition of the diary by Ikeda Kikan in the *Iwanami Library* in two volumes.

SHIO SAKAMISHI.

Library of Congress.



*Sei Shônagon, son Temps et son Œuvre* (une femme de lettres de l'ancien Japon). Avec une préface de M. Michel Revon. By ANDRÉ BEAUJARD. Paris: LIBRAIRIE ORIENTAL ET AMÉRICAINNE, 1934. Pp. 377.

This is a companion volume to *Les Notes de Chevet de Sei Shônagon*, and it developed out of a set of notes accumulated by the author in his attempts to clarify and explain the obscure allusions and the conceptions in Sei Shônagon's memoirs. Written primarily as a guide to the reader of *Les Notes de Chevet*, it will serve many a social historian of the occident as a well documented source of material connected with life in the 10th century Japanese court.

The book is divided into three parts: Part I provides as general background a rapid survey of Japanese and Chinese history, the geographical divisions of the country, the political organization, the common religious beliefs, as well as the customs, interests, and daily occupations of the members of the court. Part II is a detailed review of the same period, given up largely to lists of important persons and events. Part III presents a penetrating and sympathetic analysis of Shônagon's work and character. Extensive quotations of Japanese and Chinese poetry enable the author to illustrate in this section the cultural and intellectual ideas to which she and her associates were bred and show very specifically what sources provided much of their knowledge and what patterns formed their literary taste and style.

It is this third portion of the work which M. Beaujard's master, M. Michel Revon, marks out for special praise in his preface, and deservedly so. For while the reconstruction of the probable knowledge of a given period in such a way as to throw light upon a given character or event is not of itself a particularly original device, it is no slight feat to have gathered the almost purely literary evidence of the sort to which M. Beaujard was of necessity confined—evidence by its very nature remarkably subtle and obscured moreover by the passage of time—and then to have analyzed the material with such discrimination as to have focussed it upon a figure little known except for her memoirs, and upon those few associates, even less well known, with whom she was intimate. Thanks to his shrewd findings as to the contemporary knowledge of folk-lore,

sciences, history, and geography, M. Beaujard is able to present Shônagon against a background relatively rich. There she emerges as not so much a blue-stocking, as a woman of wit and of noticeable presence of mind in exhibiting what she knew.

It is fitting that the first comprehensive study of the background of Sei Shônagon's memoirs should have been the work of a Frenchman. The parallel between the courts of Emperor Ichijō and Le Roi Soleil springs immediately to mind. Who, moreover, learning to know well the kind of life which fostered the wit, the presence of mind, the conscious gravity, and the intellectual snobbishness of Shônagon, is not reminded of that culture responsible for certain *Précieuses* and *femmes savantes*?

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### NOTES OF THE SOCIETY

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Dr. Rudolf Meyer, Rietstahl died December 31, 1936.

Professor Moriz Winternitz, honorary member of the Society, died January 2, 1937.

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### NOTES OF OTHER SOCIETIES

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The *Revue d'Égyptologie* will resume regular publication in the year 1937. All books for review and correspondence should be addressed to the *Secrétariat de la Revue d'Égyptologie*, chez M. Raymond Weil, 4bis, rue Damesse, Paris (13°).

## ANALOGICAL CREATION AND CONTAMINATION AS ILLUSTRATED BY LAPSES \*

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ALTHOUGH thousands of analogic changes have been recorded, and although several attempts have been made to classify them and to discover their psychological basis, no serviceable classification has yet been agreed upon. The discoverers of analogic changes usually do not trouble to assign them to any particular category at all; but nevertheless it is not hard to find instances of sharp disagreement about such matters.<sup>1</sup>

That this is so is in large part due to the fact that study of analogic changes has usually been directed, not to their origin in actual speech, but only to their final results in traditional languages. The need for careful observation of the momentary lapses which form the starting point of most linguistic innovations was clearly seen by Oertel <sup>2</sup> thirty-six years ago:

Whenever we have to do with changes in the language, belief, or customs of a social body, we must at the very outset distinguish between primary and secondary changes. By primary changes I mean those which originated in, and were created by, the individual, who, therefore, plays an active part in their production. By secondary changes I mean those which, having been originated elsewhere, are adopted by the individual, who, therefore, plays a passive part. This second class of changes, is, therefore, "imitative" . . . The distinction between primary and secondary changes is of the greatest importance, because the causes for a change can only be studied where the change is primary.

In spite of this lucid statement Oertel did not go on to a really incisive treatment of the phenomena of analogy, partly because of too great confidence in the then current procedure of the psychologists and partly because relatively few lapses had at that time been recorded. Oertel did make profitable use of the one collection of lapses

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\* Presidential address delivered at the Society's Annual Meeting, Cleveland, March 31, 1937.

<sup>1</sup> How widely the best linguists may differ in their conception of analogy is seen by comparing Wackernagel, *IF* 14, 373-6 with Buck, *GR* 10, 247-9, or Leumann, *Stolz-Schmalz Lat. Gram.* 194 f. with Bloomfield, *Language* 404-24.

<sup>2</sup> Hanns Oertel, *Lectures on the Study of Language* 136-8.

then available, Meringer and Mayer, *Versprochen und Verlesen*. It was in fact Oertel's treatment of lapses that convinced me of the fundamental importance of this kind of linguistic evidence.

Eduard Hermann, *Lautgesetz und Analogie* (1931) has paid much more attention than his predecessors to the evidence from lapses, but he often neglects the distinction between primary and secondary change, and his treatment is in large part invalidated by uncritical acceptance of certain current theories.<sup>2</sup>

It seems worth while, then, to attempt a classification of analogic changes on the basis of lapses, paying particular attention to the linguistic situation that leads in each case to the lapse. We are still hampered by lack of extensive collections of carefully recorded lapses for any language except German, for which we now have Meringer's later and more important book, *Aus dem Leben der Sprache* (1908), and many pertinent observations in Clara and William Stern, *Die Kindersprache*, 4th edition (1928). For most other languages we have only random observations recorded by various scholars in widely scattered books and articles.<sup>3</sup> Even more serious is the scanty record of the circumstances under which lapses were made. The Sterns give scarcely any information of this sort, and even Meringer often abbreviates his account when he thinks that the underlying psychology is fully understood, as in case of analogic change. Under these circumstances we cannot hope to arrive at a complete and final solution of the problem.

First of all I must point out that a considerable number of the phenomena usually included under the name of analogy really exemplify the various processes of assimilation and dissimilation. I cannot here undertake a treatment of these matters, important and interesting as they are, but I must give some examples of the very numerous lapses that illustrate them. Bawden<sup>4</sup> reports *tidal wive* for *tidal wave* (p. 103), *spring ticken* for *spring chicken ten cents a pound* (p. 117), *I fool so foalish* for *I feel so foalish* (p. 118). Meringer reports *die Bürste des Fürsten* for *die Büste des Fürsten* (p. 30), *dass man die Fenster oben . . . offen haben kann* (p. 31), *ich bi viel* for *ich bin viel* (p. 32), *der Wachs geht wag* for *geht weg* (p. 63). Lapses like these, which embody distance assimilation, dissimilation, or metathesis form the great

<sup>2</sup> See Bloomfield's review, *Lang.* 8, 220-33 (1932).

<sup>3</sup> *Psychological Monographs* (Supplement to *The Psychological Review*) 3, No. 4 (1900).

majority of those collected by Meringer. Yet, although such slips are extremely common in all conversation, comparatively few of them have any lasting effect upon the language. They generally arise in a context that is either not repeated at all or repeated so rarely that it is never imitated. The accented vowel of *tidal* can affect the vowel of *wave* only in the phrase *tidal wave*; but *wave* occurs many hundreds of times in other phrases for one occurrence in this phrase. The recorded lapse *tidal wave* may have occurred just once; it can scarcely become frequent enough ever to make the community prefer the pronunciation *wive* to *wave*.

There are, however, many standing phrases of such frequent occurrence that in them an assimilation or dissimilation may occur again and again and finally be imitated until the new pronunciation is established as the norm. Examples of such common phrases in English are: *as hard as nails*, *like as two peas*, *for love or money*, *slow but sure*, *one and all*, *bright and early*, *by leaps and bounds*, *over head and ears*, *ways and means*. Many of these phrases involve alliteration or assonance: *bag and baggage*, *rough and ready*, *part and parcel*, *safe and sound*, *fair and square*, *high and dry*, *wear and tear*, and no doubt this feature is somehow connected with the assimilation so common in lapses. At any rate some of the recorded lapses affect phrases of precisely this sort, e. g. *Freunde und Freunde* for *Feinde und Freunde* (Meringer 30), *rechts und links* for *rechts und links* (113), *Gut und Blut* for *Gut und Blut* (19), *Sond und Sonne* for *Mond und Sonne* (21), *über Wand und Laster* for *über Land und Wasser* (Meringer und Mayer, 20).

It would seem to follow that many linguistic innovations which actually embody an assimilation of one member of a standing phrase to another should be definitely classed with the phenomena of assimilation and dissimilation instead of being rather vaguely assigned to "analogy." For example, French *mâle* and *fémele* might be expected to yield Eng. [meil] and [fimeil]. One can scarcely doubt that the form [fi-meil] is due to assimilation in the standing phrase *male and female*.

Lat. *pōsa* "drink" contains a suffix that does not appear in any cognate word; no doubt Latin inherited a derivative of the root \*pō- "drink" in some other suffix. This may well have been \*pōne, cognate with Umbrian *poni*, the name of a drink which is prescribed either alone or with wine at several points in the ritual and for

which wine may be substituted at other points.<sup>8</sup> Whatever the inherited meaning of this word in Latin, it evidently became the general term for drink in the phrase for "food and drink"; and so for *esca et \*pōne* we have in Plautus, Truc. 609, the gen. pl. *escārum et pōscārum*. After Plautus the word *pōsca* is specialized as the name of the ordinary poor man's drink, namely sour wine.

It has long been recognized<sup>9</sup> that standing phrases frequently show such assimilation, but scholars have nevertheless called the change analogy. So Brugmann:<sup>10</sup>

Bei dieser Art von analogischer Umgestaltung ist, was nicht übersehen werden darf, nicht nur die Bedeutung der Wörter an sich wirksam, sondern oft auch der Umstand, dass sie in der Sprache gern unmittelbar verbunden auftreten, wie *Tag und Nacht, aus und ein*. In stehenden Verbindungen macht sich überhaupt leicht der Trieb, Reime herzustellen geltend, z. B. in der Mundart von Bero-Münster *sötig und brötig* statt *sötig und brütig* "siedend heiss und brütend heiss."

Even in Hermann's<sup>11</sup> recent discussion of the subject assimilation within a phrase is carelessly grouped with analogy.

Other scholars have neglected the standing phrases almost entirely, particularly those who have conducted laboratory experiments intended to throw light on linguistic analogy.<sup>12</sup> If it is true, as Hermann says,<sup>13</sup> that Thumb and Marbe's experiments have scarcely taught us anything, it is simply because they failed to see the importance of the standing phrases. In the effort to determine the

<sup>8</sup> This ritual drink can scarcely have been so poor a thing as vinegar or sour wine mixed with water. May we assume that the Umbrians retained the old Indo-European drink, mead, in ritual use? Thurneysen's (*Glotta* I. 242-4) interpretation of *poni* as equivalent to Lat. *mola salsa* is unsatisfactory since *poni* is served in cups and since wine may, in certain cases, be substituted for it. The dependence of *pōsca* upon *esca* has long been recognized.

<sup>9</sup> So Schuchardt, *Ueber die Lautgesetze* 7.

<sup>10</sup> *Berichte d. sächs. Gesellschaft d. Wiss.* 35. 193 f.

<sup>11</sup> *Lautgesetz und Analogie* 128-35.

<sup>12</sup> E. g. A. Thumb and K. Marbe, *Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung*; Erwin A. Eger, "A Contribution to the Experimental Study of Language" (*The Psychological Review* 25. 468-487); *A Technique for the Experimental Investigation of Associative Interference in Artificial Linguistic Material* (*Language Monographs*, No. 1); David Lee Wolfe, *The Relation between Linguistic Structure and Associative Linguistic Material* (*Language Monographs*, No. 11).

<sup>13</sup> *Lautgesetz und Analogie* 81.

"associative basis" of analogy, the experimenter placed his subject in a quiet, darkened room and instructed him to respond to a word spoken by the experimenter by uttering some other word. When the word *Vater* was spoken the response was always *Mutter*, and subsequent experiments have shown that under similar conditions an English speaking subject will quite as regularly reply *mother* when the word *father* is spoken. Of course these experiments merely prove, as could have been discovered more easily by other means, that the commonest German phrase beginning with *Vater* is *Vater und Mutter*, while the commonest English phrase beginning with *father* is *father and mother*. A linguist can go a step further and predict that if the psychologists can devise some way to inhibit the responses *Mutter* and *mother*, their experiments will quite as regularly yield German *Sohn* and English *son*.

A great many standing phrases join two alternatives or two opposites: *kill or cure, more or less, rain or shine, sink or swim, first and last, day and night, give and take, to right and left, land and water, high and low*. Now it is a commonplace in chapters on analogy<sup>11</sup> to say that words are particularly likely to be associated with their opposites, and hence to be analogically influenced by them. Evidently some lapses that show the influence of a word of opposite meaning are really due to collocation in a phrase, e. g. *Freunde und Feinde* for *Feinde und Freunde*, and several others of the lapses cited above (p. 137) from Meringer. One may suspect that many of the assimilations of opposites to each other in conventional languages arose in this way, e. g. German *Tag* and *Nacht* with like vowels as against Eng. *day* and *night*.

Meringer<sup>12</sup> finds a good many lapses that show assimilation of a word to one just spoken by another person. Meringer himself once mentioned *das Stipendium von der alten Baronin*, and his wife asked: *Wie alt ist das . . . wie gross ist das?* The word *verspätet* had just been spoken, and so another person said *verspändigt* for *verständigt*. Meringer said to his four-year-old son: *Sei brav*, and the boy said: *Bleib brav . . . bleib bei mir*.

It is evident then that if two words occur frequently enough side by side in conversation they may be assimilated so often as to induce a linguistic change. This may be the history of some instances of the adaptation of suffixes to which Maurice Bloomfield called atten-

<sup>11</sup> Bloomfield's chapter, *Language* 404-24, is an honorable exception.

<sup>12</sup> *Aus dem Leben der Sprache* 59-61, 62 f.

tion in his well known article "On Adaptation of Suffixes in Congeneric Classes of Substantives."<sup>13</sup> For example, the ending *-ξ* in Greek animal names may have spread in this way. One of Meringer's lapses shows quite clearly how such things may happen. Frau Meringer: *Gib mir eine Zeitung, die gestrige oder eine andere.* Meringer: *Nein, jetzt kannst Du schon die heutige lesen* (112).

But as far as Bloomfield's material is to be explained in this way, he has classified it incorrectly; it illustrates not analogy but assimilation.

We turn now to the process to which Hermann Paul<sup>14</sup> confined the name analogy, namely the process which some scholars have called proportional analogy. Leonard Bloomfield,<sup>15</sup> in full agreement with Paul, says of this:

In most cases—and these are the ones we come nearest to understanding—the process of uttering a new form is quite like that of ordinary grammatical analogy. The speaker who, without having heard it, produced the form *cows*, uttered this form just as he uttered any other regular plural noun, on the scheme

$$\text{cow} : \text{cows} = \text{cow} : x.$$

The model set (*cow* : *cows*) in this diagram represents a series of models (e.g. *bough* : *boughs*, *heifer* : *heifers*, *stone* : *stones*, etc., etc.), which, in our instance, includes all the regular noun-paradigms in the language.

In my opinion it is a pity that Paul's sharp distinction between the two processes of analogy and contamination has been given up. The historical reason for the confusion is clear enough; it is usually difficult and often impossible to decide in which of the two ways a particular linguistic innovation arose, and so scholars have saved themselves much trouble by loosely calling both processes analogy. As we shall see, however, the lapses clearly show that at least two very different kinds of situation are involved, and sound method requires that we constantly bear them in mind, even though we must frequently acknowledge our inability to decide which of them actually occurred in a given case. We cannot, however, call the first

<sup>13</sup> *AJP* 12, 1-28. Cf. *IF* 4, 66-78, *AJP* 16, 408-34. The strictures upon these articles in Persson, *Beiträge zur indogermanischen Wortforschung* 2, 593-611, seem to be essentially sound, in spite of Güntert, *Ueber Reimwortbildungen im Atrischen und Altgriechischen* 7 f.

<sup>14</sup> *Prinzipien der Sprachgeschichte* 90-109 (1899) — Ed. 5, 106-20 (1920).

<sup>15</sup> *Language* 405 f.



process simply "analogy," as Paul did, since the vaguer use of the term has now become so common. I shall follow Oertel<sup>16</sup> in calling it analogical creation.

By way of proving that Paul and Bloomfield are essentially right in their description of the process of analogical creation I want to cite several lapses. I begin with two that I have myself observed. One of my sons when a child suffered frequently from an ailment of the ear for which the standard treatment was irrigation with warm water. Once he had some trouble with his nose, and again warm water was poured into him, this time by way of the nose. He promptly reported that he had been *navigated*.<sup>17</sup> Here there can be no doubt that the new word *navigated* was a logical creation on the model.

*ear* : irrigated — *nose* : navigated.

Not that the child could have formulated the process in that way or in any way whatever. But he knew, or thought he knew, the relation between *ear* and the experience which he called being irrigated, and now he was confronted with a similar experience that concerned his nose; the solution followed at once.

My granddaughter, at the age of four, said: "I *bate* Honey-Boy <her name for her younger brother>; I *ate* my oatmeal before he ate his." Her problem can be stated

*eat* : ate — *beat* : *x*.

There is no other possible model in the English language, and the actual operation of this model is guaranteed by its presence in the same sentence. For fear that some may doubt whether the forms *eat* and *beat* really played a role here, I must add that the children's rivalry in finishing their oatmeal was carefully taught them by their elders to prevent breakfast from lingering until it should merge into lunch. When the business of eating lagged someone would say, "I wonder whether Ann or Bradford will eat all his oatmeal first." And another would say, "I think Ann will beat today." I have no record of the conversation on the morning in question, but there is little doubt that the forms *eat* and *beat* were scattered all through it.

If any still doubt that children can produce a linguistic innovation by means of an analogical proportion, there is the Danish child

<sup>16</sup> *Lectures on the Study of Language* 163.

<sup>17</sup> This lapse has previously been reported in *QW* 20.24.

that Jespersen<sup>18</sup> tells of, who was corrected for using a strong preterit *nak* "nodded" for the correct weak preterit *nikkede*, and who thereupon produced a perfectly correct unalogical formula: *stikker stak, nikker nak*.

But such reasoning powers in children should not surprise us, since parallel problems can be solved by such stupid animals as chicks. I refer to certain experiments reported by Köhler<sup>19</sup> as follows:

A chick, trained with two grays, I and II (II being darker than I), always to choose II, will, after a while, when II and the new (darker) gray III are given, in the majority of the trials, not choose II but the unknown nuance III. The same experiments were performed on apes with size, and also with different hues of color. Several investigators have been able to confirm these experiments. We may conclude that animals react to such pairs as to wholes, either side of which has a definite character depending upon its "position" in the whole.

Here we have a number of ratios arranged in pairs as in our formulae for analogical creation, except that the middle terms are identical; thus

$$I : II = II : III.$$

In the three unalogical lapses so far considered we can say with some confidence that the new form was created on the basis of one simple proportion of four members, and probably if we had complete information a large proportion of all primary analogical creations would turn out to be of that type. As a matter of fact, however, we scarcely ever have enough information about an analogical creation that has been adopted by a language to assign its origin to a single proportion. Paul<sup>20</sup> ascribed the early Latin genitives in -i from nouns of the fourth declension to the proportion

$$animus : animi = senatus : x,$$

where *animus* and *animi* were intended to symbolize all masculines of the second declension and *senatus* all masculines of the fourth. Hermann<sup>21</sup> suggests that a better statement would have been

$$populus : populi = senatus : x,$$

<sup>18</sup> *Language, its Nature, Development, and Origin* 131.

<sup>19</sup> Wolfgang Köhler, *Gestalt Psychology* 216 f.

<sup>20</sup> *Prinzipien der Sprachgeschichte* 100 = Ed. 5, 117.

<sup>21</sup> *Lautesetz und Analogie* 76 f.

since *populus* and *senātus* are members of a common phrase. It is indeed likely that the precise proportion suggested by Hermann did on several occasions lead to the form *senātū*; but there were other standing phrases that may have suggested an equally effective proportion; e. g. *senātus legitiūsus*, *locus Albānus*. And after all, many such lapses were probably due, not to standing phrases, but to casual collocations of words as unusual and unpredictable as the collocation of *eat* and *beat* that led to the new preterit *ēate* (see above, p. 141). On the whole Paul's formula is preferable to Hermann's, since it does not seem to state the whole case. Hermann is probably right, however, in thinking that many if not most of the lapses underlying the early Latin genitives in *-i* from fourth declension nouns were due to specific formulae arising in collocations of particular second declension masculines with particular fourth declension masculines. Bloomfield needs correction when he says (see above, p. 140) that the first speaker of the form *cows* (nom. pl.) did so on a series of models including all the regular noun-paradigms in the language. It is more likely that the first speaker of *cows* was influenced by one model pair, the second by another, and so on. At any rate, we cannot assume the operation of all the possible models upon any one speaker.

Examination of another lapse may perhaps make the point clearer. An applicant for a professorship in a certain university wrote: *I am now professing Latin in <such and such a> College.*<sup>22</sup> The best guess is that he was influenced by the proportion

*teacher : teaching* — *professor : x*,

but perhaps, since he lived in a rural Southern community, his proportion was this:

*professor of religion : I am professing religion*  
— *Professor of Latin : x*.

It should be carefully noted that the effect of collocations of words upon analogical creation is a very different matter from assimilation within a phrase (see above, p. 137-9). I do not mean to suggest that in such a phrase as *senātū populique Rōmānū* the final syllable of *senātus* has been assimilated to that of *populū* as

<sup>22</sup> Previously reported in *CW* 20, 95. I have since heard of other occurrences of a verb *profess* in this sense; perhaps the instance in the text is not a lapse after all.

Eug. *female* has been assimilated to *male* in the phrase *male and female*. We cannot always decide which of the two processes has operated in a given case, but one important criterion is furnished by the question: Can a proportion be found to account for the observed change? And for the genitive *sandM* proportions can easily be found.

Hermann's recent treatment of analogy<sup>21</sup> argues that for the most part analogical proportions do not picture the real psychological process involved, and there is no doubt that many linguists would agree with him. Hermann's acquaintance with the evidence of lapses and with the learning of foreign languages convinces him that the logical process represented by a proportion does sometimes occur, but he thinks that it is not the normal method, because (p. 95) it would demand a certain degree of consciousness. ("Eine Proportion würde schon einen Grad des Bewusstseins beanspruchen.")

This, I think, is precisely where Hermann goes wrong. The lapses reported above (p. 141) from my son and my granddaughter certainly involved no reflection, no artistic preparation whatever. They seem to have been precisely on a plane with the chick's choice of the darker gray in preference to the shade which it had chosen when that was the darker member of a pair. Such an inference is by no means slow or difficult; on the contrary it is peculiarly characteristic of naïve behavior.

A striking contrast with this childlike and even subhuman process is presented by the linguistic efforts of advertisers, journalists, etc., of which Maurice Bloomfield<sup>22</sup> wrote as follows in 1895:

Adaptation, on the other hand, may be positively reflective, as e. g. in the following extract, in which the struggle between silver and gold for the control of the currency of the United States has inspired the editor of the *Atlanta Constitution* to say: "If the goldolaters and the money power propose to control, that fact cannot be made apparent a moment too soon." The editor in question did exactly as the philosopher who coined *symbolatry* after *idolatry* . . . Chemists, manufacturers of quack medicines, inventors of new explosives etc., supported by the freemasonry of their respective classes and the acquiescent public, float their *-ites* and *-ates*, *-ides* and *-ades*, with dire intent: *terrorite* and *Americasite* have been invented recently to match *dynamite*, and one feels like drawing the curtain over the indecently produce offspring of *easeline*—the *roselines*, the *blosselines*, the *pagelines*, and the *nocelines*. The banality of these processes is offset by the startling subtleness of the categories which are accentuated by an adapted suffix:

<sup>21</sup> *Lautesetz und Analogie* 72-103.

<sup>22</sup> *AJP* 16, 411.

they are often the very stuff that dreams are made of. The sinister *electrocution* reminds us that the toddling onward steps of our civilization may yield us further a *\*hydrocution*, if perchance the theory that drowning is rather pleasant than otherwise should prevail.

More recent examples of deliberately invented words are *Socony* from the initials of *Standard Oil Co. of New York*, *rimso* and *chippo* as names for kinds of soap, and *Finast* as the label for goods sold by the *First National Stores*. The periodical *Time* specializes in such ingenious words as *cinemaddict*, *cinemasculated*, *radiatorator*, *saxpert* (of Mr. Earl Carroll), etc.

Some such words superficially resemble derivatives or compounds produced in the ordinary way by analogical creation, but it is rarely possible to find a proportion that can account for them. *Tennist* "tennis player" contains a familiar suffix, but it is obviously not formed in the usual way. As soon as I tell you that it is cited from *Time* you see the explanation. It did not arise from an honest effort to speak the English language but from an itch to use a word never heard before. But I do not mean to imply that there is anything reprehensible in saying or printing the word *tennist*—if anyone wants to. The very similar word *scientist* is said<sup>22</sup> to have been invented by a professor of theology at Cambridge, and it cannot be accounted for by a proportion any more than *tennist* can.

Such words are not normal linguistic developments, but the work of artists of high or low degree. As Paul Shorey<sup>23</sup> once said, "The unconscious genius of the people no more invents slang than it invents epics. It is coined in the sweat of their brow by smart writers who, as they would say, are out for the coin."

Instead, then, of doubting the validity of Paul's proportional formulae because they imply too much art, the available evidence indicates that the artists in language are precisely the ones who are likely to coin words by other and less logical methods.

Before we go on to a discussion of contamination, it will be well to notice briefly a class of phenomena which have a certain superficial resemblance to analogical creation and have often been called "analogy," but which are really quite different.

It is usual to treat "popular etymology" in the chapter on

<sup>22</sup> Mencken, *The American Language* 569.

<sup>23</sup> *The American Language in Academy Papers: Addresses on Language Problems* 142.

analogy, since the "popular etymology" is usually evidenced only by the assimilation of one word to another. Thus Eng. *bridegroom* from OE *brydguma* (literally) "bride-man" is said to owe its second *r* to a "popular etymology" from *groom*. The term "popular etymology" of course involves a dangerous figure of speech; it seems to impute to the naïve speaker and hearer a scholar's idle curiosity about the history of words. The truth of the matter is that the ordinary man merely tries to understand what he hears, and sometimes hits upon an interpretation that the speaker did not intend. Archaic English *turtie* meaning "dove" was sometimes misunderstood as designating the marine reptile. We insure a correct understanding, but at the same time record the old misunderstanding, by saying *turtledove*.

This sort of misinterpretation is then simply an error in the linguistic analysis which forms the counterpart to the synthetic process that we have called analogical creation. The interrelation of the two will appear if we look again at the analogical creation, *nosigated*. This new form would have been impossible if the old form *irrigated* had not been interpreted as meaning "having had water poured into one's ear." The analogical creation resulted in part from the "popular etymology," but the two processes were quite distinct.<sup>21</sup>

At bottom "popular etymology" is one aspect of the reinterpretation of speech forms, to which Leumann has called attention in his important article, "Zum Mechanismus des Bedeutungswandels."<sup>22</sup>

Paul<sup>23</sup> separated contamination completely from analogy, and I think that in general he was right. By contamination Paul meant the combination of parts of two locutions to form a new whole, excluding only those that arose under the influence of a proportion. A little examination of lapses, however, shows that such combinations of two locutions arise in very different ways, and it will be convenient to restrict the term contamination to the commonest type.

Meringer reports the lapse *durch viele Jahre lang* contaminated from *durch viele Jahre* and *viele Jahre lang* (p. 73). It seems

<sup>21</sup> Paul understood the matter perfectly, and treated "popular etymology" in a different chapter. For the precise way in which "popular etymology" leads to phonetic innovation, see Paul, *Prinzipien der Sprachgeschichte*<sup>2</sup> 200 f. — Ed. 5, 220 f.

<sup>22</sup> *JP* 45, 103-18.

<sup>23</sup> *Prinzipien der Sprachgeschichte*<sup>2</sup> 145-56 — Ed. 5, 160-73.

evident that the speaker hesitated between two possible responses to the situation and then combined them.

A woman who was making arrangements for a church supper said, in response to a suggestion: *Yes, I think that will be less informal*, when she intended to say either *less formal* or *more informal*.

At a meeting of the American Council of Learned Societies, Jan. 29, 1937, a committee chairman said: *The Committee is not . . . is a young one*. He evidently hesitated between *is not an old one* and *is a young one*, but in this case a complete fusion of the two did not result. The next day another speaker, hesitating between *really*, *in fact*, and *de facto*, said: *really in . . . de facto*.

Meringer reports that a woman combined *eine junge Dame* and *ein junges Mädchen* to form *eine junges Mädchen* (p. 72).

When President Wilson said: *No man is too big to decline the presidency* he undoubtedly combined *No man is big enough to decline the presidency* and *No man is too big for the presidency*.

Precisely the same sort of hesitation often induces contamination of two words. Oertel<sup>20</sup> reports that Bishop Potter once said *evoid*, which he immediately corrected to *both avoid and evade*. Similarly<sup>21</sup> we have the German lapses *Mausch* from *Mann* and *Mensch*, *das Licht durchbringt* from *durchbricht* and *durchdringt*, *überstaunt* from *überrascht* and *erschaut*, *thumm* from *thöricht* and *dumm*, *auszüglich* from *ausgezeichnet* and *vorzüglich*.

If we restrict the term contamination to fusions of rival responses to a given complex of stimuli, it is clear that many of the examples of contamination listed by scholars will have to be classified differently. Paul<sup>22</sup> classifies here Vulgar Lat. *voster* for *vester* under the influence of *voster*, but frequent hesitation between these words is incredible. We had better assume the proportion

$$nōs : voster = vds : n.$$

Paul's next example is Gk. *μηκέτι*, for which there is an easy proportion,

$$oû : oûkéti = μή : μηκέτι.$$

Vulgar Lat. *gravis* pretty certainly owes its *s* to assimilation in the phrases, *gravis aut levis*, etc., while Lat. *nectā* results from the

<sup>20</sup> *Lectures on the Study of Language* 167.

<sup>21</sup> Meringer 71-9.

<sup>22</sup> *Prinzipien der Sprachgeschichte* 140 = Ed. 5, 162.

phrase *die nocturne*. Paul's list includes a number of other words that had better be classified elsewhere.

Rival responses are normally synonymous or nearly so, but one frequent type of nearly synonymous pairs consists of a word and its opposite combined with a negative, e. g. *young* and *not old*, *big* and *not little*, etc. This seems to be the explanation for frequent confusion of opposites reported by Meringer where one of the words does not occur in the context, and therefore assimilation is out of the question (cf. above p. 132). Meringer usually gives little or no context for these lapses and he seems to have felt no curiosity about them; for he thought they were fully explained by the Herbartian doctrine of the association of ideas. Consequently it is not often possible to be quite sure of the essential facts about them. Still there is some of his material that seems fairly clear.

Meringer said to his wife: *Weisst du wie viel Grad es hat?* She replied: *Es wird nicht wenig haben*; but in response to a question she corrected herself: *Ah, nicht viel haben* (p. 52). No doubt she had hesitated between *wenig* and *nicht viel*.

A man said: *Ich bin weit entfernt, ihn zu schätzen* (p. 52), but meant the opposite, which he might have expressed either by using an infinitive of opposite meaning or by omitting the governing clause and saying: *Ich achte ihn*.

Nevertheless this is not the whole story. Children often confuse opposites such as *yesterday* and *tomorrow*, which are not frequently collocated in a phrase; and they do it even where there is no explanation at hand in the context. It is said that children have actual difficulty in remembering which word goes with which idea, and my own experience leads me to think that this is true. In the case of the pair *left* and *right* I have all my life retained the childish tendency to confusion. When confronted with sudden need to name one of these two I am never sure which I want. I can always point to the side meant, but which name belongs to it I do not know without taking time for a little investigation. Such confusion in this particular pair is said to be fairly common among adults, and similar uncertainty about other contrasting pairs seems to be quite usual with children. Its effect is probably to be seen in American English *learn* in place of *tesch*, Gothic *gistradagis* "tomorrow,"<sup>22</sup>

<sup>22</sup> Jespersen, *Language, its Nature, Development, and Origin* 120.



Gothic *niman* "take" beside Greek *eiluo* "distribute," and Hittite *da-* "take" beside general Indo-European *dō-* "give."

In general it is even more difficult definitely to identify a linguistic innovation as an instance of contamination than as an instance of analogical creation. If a satisfactory proportion cannot be found it is worth while to search for possible sources of contamination, but one can be confident of his results only in exceptional circumstances. Thus Lat. *urbānus* cannot be derived from words like *Rōmānus* by any possible proportion. But if we assume that some prehistoric predecessor of *urbānus* (any *\*urbicus*) was as nearly synonymous with *Rōmānus* as *urbs* was in historic times with *Rōma*, then hesitation between this *\*urbicus* and *Rōmānus* may well have yielded *urbānus*. Similarly the Greek comparatives in *-αρτερος* from adjectives in *-ωρ* cannot be accounted for by a proportion, but they may have originated in hesitation between such a pair as *\*εὐδαμωρόωρ* (or *\*εὐδαμώρτερος*) and *εὐχάαρτερος*. The great weakness of such constructions is that they must forever remain purely hypothetical.

None the less it is important for the student of etymology and phonology to bear in mind that two words may be fused if a speaker hesitates between them. In the realm of syntax contamination is much more practically important, since the lapses themselves get into our texts; the traditional term of Greek and Latin grammar is *anacoluthon*.

Certain lapses seem to involve both analogical creation and contamination. This sort of thing is quite familiar in the speech of children. The Sterns<sup>24</sup> record *gingle* as spoken by a four-year-old apparently by contamination of *ging* with *gekts*. So *drunked* seems to result from *drunk* and *drinked*, *foets* from *feet* and *foots*. The importance of this process for historical linguistics is illustrated by Lat. *iecinoris* "of the liver" from inherited *iecinis* and analogical *\*iecoris* and similarly by *itineris* "of the road" from inherited *\*itōnis* and analogical *\*iteris*.

There seems to be no theoretic difficulty in assuming that an inherited form and a neologism due to distance assimilation may be contaminated, and it would be easily possible to explain many lapses in this way. A second person form is sometimes substituted for a first person form if that particular form has recently been spoken.

<sup>24</sup> *Die Kindersprache* 141.

Thus Meringer cites from a seven-year-old: *Du sagst "nach Haus,"* "nach Haus" *sagst ja auch ich* " (p. 115). Hence when Meringer's interlocutor had said: *Ich seh*, and Meringer responded: *So, Du sehest . . . siehst* (p. 63); his form *seht* may either be due to analogical creation or to contamination of assimilative *Du seh* and traditional *Du siehst*.

I mention this possibility chiefly because it seems to furnish the only solution to a riddle of Greek grammar. Attic εἰμί "I am" has been thought to contain  $\bar{\epsilon}$  lengthened on the loss of  $\sigma$  before  $\mu$ , although the writing with  $\alpha$  was known to be uncomfortably early for such a theory. The recent discovery of an inscription of the end of the eighth century with the form εἰμῖ<sup>44</sup> shows beyond question that in this word  $\alpha$  is a true diphthong. I suggest that it is a contamination of the second person εἶ used by assimilation in the value "I am" and the inherited εἰμῖ.

A study of the relatively few lapses that have been recorded indicates, then, that a good many of the linguistic innovations that have been ascribed to analogy are really instances of distance assimilation. Of the remainder, some certainly result from a situation that sets a problem in proportion, and there is reason to believe that this process is very common and of great importance in linguistic history. We have called it analogical creation. Very many lapses are due to rivalry between two possible responses to a complex of stimuli. Most such lapses remain isolated and therefore have no lasting effect upon language; but repetition of a particular lapse in a common phrase or in a common conversational sequence occasionally leads to a linguistic innovation. This process we have called contamination.

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<sup>44</sup> Shear, *Hesperia* 5. 33.

# A CONDITIONED SOUND CHANGE IN RAS SHAMRA<sup>1</sup>

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THE ALPHABET of Ras Shamra included three signs for ' , one of which was used when an a-vowel followed, i. e. for 'a, 'ā, the other for 'i, 'ī, 'ē, and the third for 'u, 'ū, 'ō.<sup>2</sup> But ' without following vowel also occurs in the language, and there has been considerable question as to how such ' was represented.

Bauer's suggestion<sup>3</sup> that any one of the three signs could be used indiscriminately for vowelless ' is inadmissible, for a definite relation usually exists between the ' -sign used and the morphological vowel of the syllable in which it occurs: *igra* for 'igra', *ša* for 'ša'; *bīr* for *bī'ru* (T<sup>a</sup> 2. 2), *mā* for *mā'lu* (T 23. 9), *yīṣp* for *yī'tasipu* (H 17); *rum* for *ru'mu*.<sup>4</sup>

Ginsberg,<sup>5</sup> on the other hand, maintains that all vowelless ' were represented by the ' -sign i. Against this interpretation are arrayed many vowelless alephs written with a, ā:

a: *rum* (I iii 22, 37)<sup>6</sup> for *ru'mu* (*rumm*; A vi 18, etc., for the plural *ru'muḥ/ḥu(a)* is not a case in point).

<sup>1</sup> This paper was read at the meeting of the Society in 1936, but I have held it up in the hope that further material from Ras Shamra would clarify some of its uncertainties.

<sup>2</sup> Friedrich, ZA, N. F. 7. 305 (1933); Ginsberg, *Turbis* 4. 381 (1933); Albright, JPOS 14. 108-9 (1934). The three alephs are here transcribed a, i, u, it being understood that they always represent ' with the vowel, thus 'a or a', etc.

<sup>3</sup> Hans Bauer, *Die alphabetischen Keilschrifttexte von Ras Shamra* (1936) p. 66-7.

<sup>4</sup> Bauer's further confusion of *tāu* and *tāa* is quite unnecessary: *tāa* represents *tāā'u*, the preterite form used throughout, while *tāa* occurs only in jussive use, as will be seen below.

<sup>5</sup> In *Turbis* 4. 382, *Orientalis* 5. 176 (1936).

<sup>6</sup> The Ras Shamra tablets are referred to as in J. A. Montgomery and Z. S. Harris, *The Ras Shamra Mythological Texts* (*Memoirs of the American Philosophical Society* IV 1935) p. 43-4, 85, 131. Most important tablets published since: F (*Syria* 1935, 247-66), G (Ch. Virolleaud, *La légende phénicienne de Danel* [Mission de Ras Shamra I] 1936), H (Ch. Virolleaud, *La légende de Heret* [Mission de Ras Shamra II] 1936), I (*Syria* 1936, 150-73), J (*Syria* 1936, 209-28), K (*Syria* 1936, 335).

- mad* (D iii 2) for *mu'di* (noun in construct case: *bmud sin*, as against the adjective *ma'du* in 'r *mid* T<sup>2</sup> 2. 13, *wyāhp mid* T<sup>2</sup> A, *mid ksp* B v 77; *mad* H 88 is of uncertain value).
- yuhd* (B iv 16) for *yuhadu* (Qal Passive).<sup>7</sup>
- yuhb* (D v 18) for *yuhabu* (Qal Passive).<sup>8</sup>
- a: *bat* (G 1. 213-4) for *bā'tā* (pl. 2 m. ag.; for form, see below).
- ša* (B viii 5, D v 13, H 75) necessarily for *šā* (imperv. m. ag.).<sup>9</sup>
- šā* (D i 22) perhaps imperative *šā'a*.
- ygra* (B vii 47) and *igra* (C1) are jussive by context: *yigra'*, 'igra'.
- tā* (D ii 16 and, in different context, G 1. 99) seems in each case to be jussive *tā'a*, with the force of a command.<sup>10</sup>
- wymā* (F i 37) and *lyā* (E 6, G 1. 75, 78 (= ?), 113, 141; *wyā* E 30?) are probably not jussives but Short Preterite *wyimā'a*, *lā yā'a*.<sup>11</sup>

The evidence for the reading *lā yā'a* involves a slight excursus into the vocalization of the verbal system. In Ras Shamra there is observable a correlation between the vowel of the preformative and

<sup>7</sup> While this is not certain, the only way to suit form and context seems to be to take it as a Passive of the First Stem, with perhaps the meaning 'lighted'; cf. Albright, *JPOS* 14.121; the *šā'a* following should be taken as a Šafel infinitive in complement to *yuhd*.

<sup>8</sup> No other verbal form seems to fit here; the unexpected use of a passive here may be connected with the stative force of the root itself.

<sup>9</sup> The form *šā* (C 54, 65) represents the plural *šā'a*. C 65 should be read *šā 'dā tē mdr qdā* 'hear ye offering into . . .'; for the use of *tē*, cf. *tē pāk* parallel to *qdā* 'before him' B v 107-8.

<sup>10</sup> Ginsberg's reading of some of these forms as subjunctive-cohortative, e.g. 'igra'a, is unsatisfactory for these cases, some of which are clearly in the syntactic position of the West Semitic jussive.

<sup>11</sup> On present evidence, Ras Shamra seems to have had two preterite forms, a common *paqtulu* used for narrative: *yā* for *yā'a* (D vi 22, rt. uš'), *igly* for *iglyā* (i, B iv 23, rt. gily, where the *y* shows the presence of the final vowel); and a Short Preterite, with no inflectional vowel, preserving the older Semitic form and used chiefly after *wa-* 'and', *lā* 'indeed': *wyā* for *wyā'a* (A i 21, rt. wy), *lāyā* for *lā 'āyā'a* (E 2, rt. yā'). In addition there was a Present, which was also a preformative conjugation but with as yet unknown vocalization; with a jussive (apocope) perhaps similar in form to the Short Preterite, as in *yigra'*, the Afel *yāpā'* (A v 4), *yā* for the Nifal *yābā'a* 'let there be built' (B iv 62, rt. bay); there was also an energetic in *-an*. Albright points out a subjunctive in *-a* which seems to be used after the particle *lā*, *h-*, *JPOS* 14.115. For a somewhat different analysis of the tenses, see Ginsberg, *Orientalia* 5. 179, 181.

that of the second root syllable. Where the stem-vowel is *u* or *i*, the preformative has *a*: 'am<sup>h</sup>uk<sup>u</sup> (A i 34), 'aššib<sup>u</sup> (Ia, G 2 ii 16), 'arid<sup>u</sup> (Iy, D vi 25), 'asit<sup>u</sup> (IIwy, B v 123), 'abb<sup>i</sup>iy<sup>u</sup> (IIIy, G 1.111).<sup>12</sup> Where, because of a laryngal, the stem-vowel has been changed to *a*, the preformative changed to *i*: 'Ṭak (B vii 45), 'šl<sup>h</sup>ak<sup>a</sup> (H 235), 'im<sup>h</sup>ak<sup>a</sup> (G 1.196), 'iqq<sup>h</sup>ak<sup>a</sup> (Igh, H 204), 'ida' (y<sup>h</sup>ā', A iii 8).<sup>13</sup> Laryngals effected this change to *a* in all verbs except IIwy roots, where the vowel was long: 'aššāḥ (D ii 21), 'awāḥ (A iii 17). In the root *gš'*, therefore, the stem-vowel must have become *a*: y<sup>h</sup>gšā'.<sup>14</sup> The forms *ygsi* (H 85, 87, 100) and *gsi* (G 3.24) do not necessarily conflict with this reading, for they are Present, jussive by context, and their *i*-vowel may be the vowel of the Present.

It is therefore clear that *i* does not represent all vowelless alephs; the choice of sign was determined by the neighboring vowel, following or preceding the ' , so that the *a*-aleph could represent 'a or *a'*, and so on. This was the suggestion of Friedrich,<sup>15</sup> and has been amply upheld by the evidence which has appeared since he made it. It will be seen that every form cited thus far is consistent with this reading.

When the whole material is reviewed, however, Friedrich's reading is found to leave a residue of apparently inconsistent forms, written with *i* although the vowel preceding the ' is morphologically *a*: *sin* for \*šā'n<sup>u</sup> (B vi 41), and so \*rā'suk<sup>u</sup> (A i 32), \*mā'd<sup>u</sup>; *tāḡd* for \*tā'ḡad<sup>u</sup> (A ii 9), and so \*yā'ḡad (T<sup>2</sup>B), \*tā'bul<sup>u</sup> (A ii 35), \*tā'supuk<sup>i</sup> (G 1.86), \*yā'pē (H 83), \*tā'kulānā (? if 2 f. sg., F i 10); *tāḡān* for \*tāḡā'nā (T 2.14). At a time when many of the texts used here were as yet unpublished, I showed this material to Professor Speiser. He suggested immediately the apparent

<sup>12</sup> The derived stems also have *a*-preformatives: Piel 'abbāqif<sup>a</sup> (A iv 44), Afel 'aššiqub<sup>a</sup>, Šafel 'aššapirub<sup>a</sup> (both G 2 vi 28), but the *t*-stems have *i*: 'atāḡiq<sup>i</sup> (B ii 24), 'štatuk<sup>i</sup> (rt, š-t, F ii 68). The vowel of the second root syllable in all the derived stems is *i*: *t*-stems 'štat<sup>i</sup>ru (G 3.15), yittak<sup>i</sup> (jussive, T 2.17), Piel 'am<sup>i</sup>id<sup>i</sup> (H 58), Afel yam<sup>i</sup>ā', Šafel 'aššiq<sup>i</sup>hum.

<sup>13</sup> This correlation in accordance with Barth's law was shown by Ginsberg as early as 1933 in *Tarbis* 4.182. Barth's law, in *ZDMG* 49.4 (1894) is substantiated by the unequivocal evidence from Ras Shamra.

<sup>14</sup> The Hebrew *ygsē* is a later development, cf. G. Bergsträsser, *Hebräische Grammatik* II § 26n (1929).

<sup>15</sup> *ZA* N.F. 7.307 (1933); Albright, *JPOS* 14.108, n. 37a.

existence of a regular sound change, and the possible contacts with similar changes in Akkadian and Aramaic. The writing of all these forms can be explained on one assumption: that there had taken place a sound change of  $a > e$  before '.<sup>10</sup> This would yield *šēnu*, *šēhuda*, *tūtē'nā*, and so on, written with i-alepha.

There remains but to fix the conditions of the change. It did not take place in *šo'*, *yigra'*, and the words of similar construction above; therefore it must have been limited to medial syllable-closing '. Nor did it take place in *bā'tā*,<sup>11</sup> whence we may judge that it was limited to short  $a$ . There are a few other forms which may preserve the combination  $a'$ , in particular *yakl* (R v 103) "he gave to eat", and *tarša* (A ii 14) "would you desire." The former is certainly not Qal, and more probably Afel than Piel; the latter is Present and its vowels therefore uncertain. If *yakl* is Afel *ya'kālū* (if the second vowel was long at this time), and *tarša* represents *ta'rišānā*, we may have to posit that only accented  $a'$  changed to  $e'$ ; in that case it would be necessary to assume that *tiktā* above is not 2 f. eg., but perhaps an energetic, with the accent on the  $a'$ .<sup>12</sup>

It is possible, therefore, to conclude that in the speech of Ras Shamra before 1500 B. C. there had taken place a sound change of short (accented?)  $a' > e'$  when the ' was followed by a consonant (i. e. medial).

While a more definite connection of this change with those of

<sup>10</sup> Z. S. Harris, *A Grammar of the Phoenician Language* (1933) p. 37.

<sup>11</sup> That *bā'tā* "you have come" has long  $a$  (which is required for differentiation from the  $a$  which, in such circumstances, became  $e$ ) is justified on several grounds. In the preterite, the stem vowel of these IIcy verbs was long, as was seen above from the fact that they did not become  $a$  in the presence of laryngals. The reasons for considering the vowel of the perfect as long are given by Landsberger, *OLZ* 1926, 976, also H. Bauer-P. Leander, *Historische Grammatik der hebräischen Sprache* (1922) p. 301 (where the derivation from \**gawwā* is unacceptable); against this Bergsträsser, *Heb. Gram.* II § 28v, suggests that the vowel was short and was lengthened in Hebrew. Cf. also the Amarna form *naḥṣi* (rt. n-ḥ) in Amarna letter 147. 56, which has been explained as representing South Canaanite \**nāḥṣi*; Dhorme, *RB* 13. 56 (1911); Böhler, *Die Sprache der Amarnabriefe* (Leipziger semitische Studien 5) p. 66; against it Bergsträsser, *Heb. Gram.* II § 28 f.

<sup>12</sup> Other forms do not come into consideration here: *gahp* and *garx* (J39) are Piel, as is probably *taḏm* (R 26); *taḏm* *tū/ḏm* (G 1. 204) is unclear as is also the group *ispi* (D i 5), *ispa* (A v 29); *raḥm* (D i 3) is, of course, the plural *ra'adīm*.

neighboring languages is as yet premature, certain similarities will bear investigation.

In Akkadian, *i* and *u* with following or preceding laryngal, became *î*, *û*, or remained, infrequently, *i*, *u*, and the like (all laryngals becoming *'*): *limu*, *k'mu*; *bûru*, *mûdu*. However, *a* in such position, while it sometimes yielded *â*, and rarely *a'*, usually became *ê* (never *e'*). The change to *ê* took place regularly if the laryngal was *'* or *g*, sometimes, if it was *'*, *h*, *h*. In the forms which changed to *ê* there are no variants preserving the *'*: *rêšu*, *bêlu*; i. e. the *'* was absorbed more regularly into the *a* than into the other vowels (*a* included), which is natural, for the *a* must have had the greatest affinity with the laryngal, since it was a product of its influence. Because of this complete absorption of the *'* in the *a* < *a*, it is difficult to discover whether the change was directly *a'* > *ê*, *aḥ* > *ê*, and so on (coalescence of the *'*, *h* with the *a*), or *a'* > *e'* (> *e'*) > *ê*. The latter appears phonetically more probable. Some external evidence exists in the forms in which the laryngal was probably absorbed not into the vowel, but into the following consonant, or in which the laryngal was initial and later dropped from speech: *teppuṣ* ('*pē*), *terriṣ* ('*rē*); *epēšu* from rare *epāšu* < *\*apāšu*, *erēšu* < *\*arāšu*, Permansive *erib* < *\*garib*. Here, obviously, the *a* was changed to *e* under the influence of the laryngal, but without involving its absorption. Within the limits noted above, then, Akkadian reveals a change of *a* before or after a laryngal to *e*, and only later to *ê* with the absorption of the *'*.

In Aramaic, the change of medial *a'* > *ê* may be seen in all dialects: <sup>19</sup> Syriac *uḡḡul*, other dialects *yēḡul*, <sup>20</sup> *rēṣā*, <sup>21</sup> and so on. The

<sup>19</sup> Under certain conditions *a'* becomes *ê*; the exact limits of the changes to *ê* and *î* are not entirely clear; cf. T. Nöldeke, *Kurzgefasste syrische Grammatik* (1898) p. 74; H. Bauer-P. Leander, *Grammatik des Biblisch-Aramäischen* (1927) p. 59.

<sup>20</sup> The I' verb forms could be explained as < *\*uḡḡul*, etc., since the preformative vowel changed from *a* to *i* in the strong verb. However, the difference between *uḡḡul* and the West Syriac *uḡḡar* < original *\*uḡḡar* suggests that we have here a direct development from *\*uḡḡul*; cf. G. Broekmann, *Grundriss der vergleichenden Grammatik der semitischen Sprachen* I (1908) p. 562 g.

<sup>21</sup> Bauer-Leander, *Gram. Bib.-Aram.* p. 60, explain the *a* here as assimilation to the *î*; however, it is when followed by consonant that *î* has this effect, Nöldeke, *Kurz. Syr. Gram.* p. 32. Their derivation, *ibid.*, of *uḡḡāḡ* < *\*uḡḡāḡ* seems doubtful; the conjunction remained *wa-*.

letter aleph was used in Syriac as a vowel letter for *a* in open syllables.<sup>22</sup> The occurrence in Syriac of traces of *a* > *e* under the influence of ';<sup>23</sup> make it probable that here, as in Akkadian, the history was *a*' > *e*' > *i*.

In Hebrew, under varying conditions, *a*' changes to *i*, with absorption of the glottal stop, or remains *a*': *rās* < \**rās* < \**ra's*-, *māšā* < \**maga*-, *ma'āḥāl*. There are, however, groups of forms which exhibit an *e*-vowel in the masoretic vocalization of the Bible. Of these, the III' verbs with *e*-vowel are explained as new constructions on the analogy of the III<sub>ay</sub> verbs.<sup>24</sup> The imperfect *yā'qol* forms of I' verbs (by the side of *yāḥal* imperfects) may be explained as late formations on the analogy of the late *yiqtol* imperfect<sup>25</sup> (*yiqtol* < *yaqtol* by change of *a* > *i* in closed unaccented syllables). In the Babylonian vocalization, these verbs regularly have long *ē*: *tēsef*;<sup>26</sup> since the above change of *a* to *i* is much less frequent in the Babylonian vocalization, it is probable that we have here a (dialectal) change of *a*' > *e*' > *i*. In addition to this group, the Tiberian vocalization itself leaves a number of forms which cannot be easily explained except by a change from *a*': e.g. *ṣāḥā* (written with aleph) < *ra'āḥ*, *ṣāḥā* (usually written with aleph).<sup>27</sup>

It would appear that in some of the northern dialects of Hebrew which existed by the side of the Jerusalem (Judaean) dialect, *a*' changed to *e*' under certain conditions; the unexplained forms above may represent inter-dialectal borrowings into Judaean Hebrew or into the usage of the masoretes.<sup>28</sup>

The following conclusions may now be stated: In most of the known Semitic languages of the northern area, in Akkadian, Aramaic, Ras Shamra, and northern dialects of Hebrew, under conditions which differed in the various languages, the pronuncia-

<sup>22</sup> Nöldeke, *Kurz. Syr. Gram.* p. 32.

<sup>23</sup> Nöldeke, *Ibid.* p. 24, 45; Brockelmann, *Syrische Grammatik* p. 31.

<sup>24</sup> Bergsträsser, *Heb. Gram.* II § 20b.

<sup>25</sup> Bauer-Leander, *Hist. Gram. Heb.* p. 347; Bergsträsser, *Heb. Gram.* I § 15d (1918).

<sup>26</sup> P. Kahle, *Masoreten des Ostens* p. 185.

<sup>27</sup> Against Bergsträsser, *Heb. Gram.* I § 15d.

<sup>28</sup> For dialect borrowing, see L. Bloomfield, *Language* (1933) p. 476-87. This borrowing has no relation to the position of Bauer-Leander, *Hist. Gram. Heb.* p. 15-26, that Hebrew was a Mischsprache, for they refer to a confusion of large morphologic classes developing from two distinct divisions of the Semitic language group.



tion of *a* before ' inclined toward *e*. The ' was later absorbed into the vowel, yielding *ê*, but in the Ras Shamra inscriptions the *e*' stage is preserved. This must be understood not as a weakening of the ', but as an anticipatory assimilation of the *a* to it. While it is true that the only distinctive element in the ' is the glottal stop, the other organs of speech necessarily had characteristic attendant positions during a typical pronunciation of ', and it is the anticipation of these non-distinctive positions which must explain the change in quality of the preceding *a*. The change did not yield a new phoneme *e*, because this was but an alternant of the *a*-phoneme;<sup>29</sup> the use of the i-sheph in Ras Shamra for this sound merely indicates that it was sufficiently different from the usual Ras Shamra *a*.



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<sup>29</sup> See Sapir, "Sound Patterns in Language," in *Language*, 1, 43-4 (1925).

## THE RÔLE OF CYRUS IN DEUTERO-ISAIAH<sup>1</sup>

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IT IS UNFORTUNATE that so much critical analysis of Deutero-Isaiah has proceeded from the assumption that it was written after the fall of Babylon.<sup>2</sup> This has tended to shift the attention from one portion at least (caps. xl-xlviii) which must have preceded the entry of Cyrus. The most striking proof is seen in xlv: 1, the familiar line beginning "Bel boweth down, Nebo stoopeth." Obviously, this predicts the end of the dominion of the Babylonian gods, on the capture of the city by Cyrus. But it is equally obvious that the prophet cannot be speaking here of a *fait accompli*; for actually, it never occurred. His assurance (xlv: 1-2) <sup>3</sup> that Cyrus will remove the images of Bel and Nebo, thus refusing to acknowledge their sovereign claims, is proved false by the conqueror's own statement that he is king by virtue of his chosen sonship of Marduk. It is he whom Marduk has chosen, according to the author of the *Cyrus Cylinder*,<sup>4</sup> to be "a righteous prince according to his heart's desire who would grasp his hands." But Deutero-Isaiah makes substantially the same assertion on behalf of Yahwêh: "Thus saith Yahwêh unto his anointed, unto Cyrus whose right hand I have taken" (xlv: 1).<sup>5</sup> There is an obvious and complete contradiction here between what the prophet expected and what actually happened.

<sup>1</sup> A paper read before the Middle West Branch of the American Oriental Society, Chicago, March 27, 1936. I must acknowledge here the kind and invaluable assistance of my two teachers, Professors F. W. Buckler and H. G. May, without which this paper could not have been written.

<sup>2</sup> As early as 1870, Cheyne (art. "Israel," *Enc. Britt.*, ixth ed., pp. 377-384) recognized that caps. xl-xlviii "evidently form a section by themselves," which has "one leading idea—the great crisis impending over Babylon and Israel. Babylon and her gods must fall, that Israel may rise again with the glorious function of giving a religion to the world."

<sup>3</sup> Cf. American Revised Version.

<sup>4</sup> Dougherty: *Nabonidus and Belshazzar*, text of *Cyrus Cylinder*, lines 11b-15 (p. 170).

<sup>5</sup> Heb. text:

בְּיָמָיו יִתֶּן לְאֶשְׁתּוֹ אֶשֶׁר הִחַרְמִי בַּיָּמִין

One technical difference, however, between the Babylonian rite and the Hebrew writer's phrase must be noticed. In the former (see note 4) it

But the passage, "Bel boweth down, Nebo stoopeth," is important not only for its bearing upon the problem of dating the prophecy. Its major significance lies in the fact that it is a contemporary expression of the conflict between the claims of Babylon and the anti-Babylonian opposition in this age.<sup>6</sup> For many years after Babylon ceases to be a major political power, the idea of Babylon forms the touchstone of whole policies of empire. Furthermore, recognition of Bel becomes a mark of Achaemenid policy and a guarantee of Achaemenid success. The defeat of Xerxes I, successor of Darius, by the Greeks, is laid to the fact that he destroyed the temple of Marduk and despised that deity.<sup>7</sup> It remained for Alexander the Great to restore Marduk, and the tradition preserved in Arrian and Pseudo-Callisthenes<sup>8</sup> asserts that Alexander, following the example of Cyrus, restored the temple of Marduk—and rebuilt the tomb of Cyrus as well.

The case of Xerxes reflects the pro-Babylonian side of the picture. For the other side we must turn to the lands flanking the Mesopotamian region, of which Babylon is the centre, namely Palestine and Elam.

First, let us consider the Jewish position. In the prediction of the exile of the idols of Bel and Nebo, there is reflected a thorough-going and traditional hatred of Babylon and her gods, a hatred that permeates the whole of Jewish history.<sup>9</sup> The way in which this

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is the King who grasps the god's hands; but Deutero-Isaiah reverses the order, so that it is Yahweh who grasps Cyrus' hand. This might conceivably have the rhetorical purpose of emphasizing the prophet's theory that Yahweh has chosen Cyrus. Another noteworthy point is that this is the first instance in the O. T. where the phrase is applied to divine election. This seems a very strong indication that Deutero-Isaiah has actually taken over the idea underlying the familiar Babylonian ritual and applied it here to the choice of Cyrus by Yahweh.

\* This conflict in its earliest stage has been fully analyzed by Father Eric Burrows, S. J., in his essay, "Some Cosmological Patterns in Babylonian Religion" (*The Labyrinth*, ed. S. H. Hooke, pp. 42-71). He asserts (pp. 52-59) that the Hebrew story of the Tower of Babel is a "polemic against Babylonian pretensions" as these were expressed in the ziqqurat of Babylon. M. Jastrow, in 1911, developed the same position in his *Religion in Babylonia and Assyria* (see esp. pp. 298-299 for his treatment of the attitude of the Hebrew writers toward Babylon).

<sup>6</sup> Cf. Herod., I: 183; Ctes., *Exc. Pers.*, 22; Arrian, vii: 17.

<sup>7</sup> Arrian, *Anabasis* (ed. Roos) VI: 29: 1, 4 ff. (C. 67); The *Pseudo-Callisthenes* (trans. Budge), Bk. I: 12; Bk. II: 18.

<sup>9</sup> Cf. above, note 6.

feeling influences Deutero-Isaiah in particular will be observed throughout this discussion.

The contradiction between the prophet's statements and the actual events which followed the triumph of Cyrus not only in the passage already cited but also in others,<sup>10</sup> forces us to the explanation that they were written during the period preceding the fall, indeed before the siege, of the city, at the time when the news came that Cyrus, having completed his conquests of Media and Lydia, had resolved to advance against Babylon. The problem immediately at hand is the prophet's use of the rôle of Cyrus as the leader of the anti-Babylonian forces.

First of all, it must be noticed that the writer, a devout Jew, adopts a gentile, a prince of the *gôylm*, as the Messiah of Yahwêh. This constitutes a paradox in Jewish thought that cannot readily be resolved. Perplexing as this paradox may be, however, it would have been still more surprising had Deutero-Isaiah written in this way after Cyrus had grasped the "right arm" of Bel Marduk.<sup>11</sup> In fact, the whole tenor of the passage points rather to the prophet's expectation that Cyrus is even about to acknowledge Yahwêh as his god. That is the only possible meaning of the passage xli: 25b as it stands, referring to Cyrus as one who "will call upon my name"—*אֶקְרָא בְּשֵׁמִי*. The passage in its present form has given rise to considerable perplexity. But if the tense of the verb is left in the simple future, and the pronominal suffix "my" is retained (*pace* Duhm),<sup>12</sup> we have: "he will call upon my name."

<sup>10</sup> For instance, the whole thought in Deutero-Isaiah which is first expressed in xli: 15-18, namely that Israel shall be made a "threshing-sledge" (*תְּהִי כְּמִשְׁכֵּל עֹשֶׂה*) that will "thresh mountains and smash them" (*תִּדְבֹשׁ הָרִים וְתִסֵּס אֹתָם*) is obviously the result of a glowing hope that was never fulfilled. If Deutero-Isaiah had written *after* Cyrus' conquest, he would have realized that the only blessing to Israel which the conquest could bring was restoration to Palestine and the reestablishment of the Temple. But in the passage I have cited, the prophet clearly declares that Yahwêh, working through the deliverance of Cyrus, is to make of Israel a mighty nation in a temporal as well as a spiritual sense. Such a prophecy, obviously, must have been made before the facts resulting from the restoration were realized.

<sup>11</sup> See above n. 5.

<sup>12</sup> Duhm's comment (*Das Buch Jesaiah*, p. 300) is a representative expression of the theory that the passage must be emended to read *אֶקְרָא בְּשֵׁמוֹ*—"I will call him by name." There is no variant reading or other version as evidence for this emendation; but a still weightier argument for accept-

And that is exactly what Deutero-Isaiah, writing in the expectation of Cyrus' entry into Babylon, almost certainly intended to say.

Having established the priority of the prophecy to the fall of Babylon, we must turn now to the problem of why Deutero-Isaiah looked to Cyrus to achieve the overthrow of Marduk and Nebo. The first reason is geographical. Cyrus came from Anshan in Elam, from the east (𐎶𐎵𐎶𐎵𐎶𐎵𐎶𐎵) of the Babylonian Empire. The foundation of his kingship rested upon the possession, not only of Elam, but of Sumer and Akkad.<sup>13</sup> Prior to Cyrus' conquest of Media, Persia was under the protection, not of Bel Marduk, against whose kingdom Cyrus is marching, not of Ahura Mazda, the protector of the Median (his grandfather's) realm;<sup>14</sup> not of Yahwéh, but of Mithra. With the capture of Media, however, by the young king of Elam, Sumer, and Akkad, there was effected a fusion of the cults of Ahura Mazda and Mithra (which may have been in process for some time as a result of the earlier relations of the Median and Persian courts).<sup>15</sup> Moreover, there may be detected

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ing the text as it stands is xlv. 3, where the purpose of the "calling" is, in part, to acquaint Cyrus with Yahwéh.

<sup>13</sup> The title, "King of Sumer and Akkad," was one of great antiquity at the time of Cyrus, having been first assumed by Ur-Nammu, founder of the third dynasty of Ur, c. 2200 B. C. The Avesta never lets us forget that the Glory (hvareñ) belongs to the Aryan race, and Azi Dabák, the arch fiend of Iranian literature, becomes the mythological protagonist of the Scythians in their fiendish endeavor to seize "the Glory that cannot forcibly be seized." A. G. and E. Warner, in the introduction to their translation of The Shikhsámah of Firdausi (Vol. I, pp. 54-55) give an elucidating paragraph to this point.

So then, in Cyrus' eyes the possessor of the Royal Glory must be *ipse facto* the "King of Sumer and Akkad"—in other words, the legitimate ruler of the whole Euphrates valley.

<sup>14</sup> E. Meyer in his article on Astyages in *Enc. Britt.* (xith ed., Vol. II, p. 821) cites the fact that "the Armenian historians render the name Astyages by Ashdubak, i. e. Ashi Dabaka (Zohak), the mythical king of the Iranian epics, who has nothing whatever to do with the historical King of the Medes." But he does not tell us why there can be no possible connection. I would suggest that the tradition preserved by the Armenian historians is not accidental, but that Astyages, by his memorable attempt, which is attested by both history and legend, to destroy Cyrus at birth, came eventually to be identified with the Azi Dabák of the Avestas—or, indeed, it was he who adumbrated the mythical figure.

<sup>15</sup> Xenophon (*Cyropaedia*, Bks. I and II) in his account of the boyhood of Cyrus gives us our most detailed record of this stage.

It is possible that the passage in Zamyád Yasht, VII, 25b (S. B. E. Vol.

a political settlement, which we see later reflected in the settlement of Darius after the rebellion of the false Smerdis, whereby the Magi, as the hereditary guardian priesthood of the Median religious rights, now found themselves entrusted with the privilege and duty of recognizing the true possessor of the royal Glory.<sup>47</sup> The fusion, whether in process or already accomplished at the time of Cyrus' conquest, relieves us of the necessity of attempting a distinction between the Perseu-Mithraic and the Medo-Zoroastrian elements in his position.

So it is possible for us now to turn to a non-Jewish interpretation of the rôle of Cyrus, which in many ways constitutes a remarkable parallel to Deutero-Isaiah as representing the other strand in the anti-Babylonian tradition—the Persian.<sup>48</sup> This interpretation is found in the *Zamyâd Yast*, which most significantly stops at Cyrus<sup>49</sup> in its narrative of the struggle which began at creation for "the Glory that cannot forcibly be seized." Since Cyrus (*Burwakh*) is the last human figure to be named in that song of kingship (*malikutâs*) of the Great King, and since this theological tradition persists, to be included in the later development of Persian *Shâhnâmâh*,<sup>50</sup> we are justified in this deduction: that the tradition

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xxiii) refers specifically to this fusion: "Then Mithra seized that Glory, Mithra, the lord of wide pastures, whose ear is quick to hear, who has a thousand senses. We sacrifice unto Mithra, the lord of all countries (italics mine), whom Ahura Mazda has created the most glorious of all the gods in the heavens." If by this time Mithra had become, not only the god of Elam, but "the lord of all countries," then the Medo-Persian fusion which I suggest must have been completed on the theological basis set forth in the rest of the above passage. The process, then, by which this fusion is accomplished is by making Mithra (the native deity of Cyrus) the god of all the lands Cyrus has conquered, as well as the faithful minister created by Ahura Mazda. It is by the same process that Deutero-Isaiah seeks to establish the eccumenical supremacy of Yahweh.

<sup>47</sup> This incident is fully dealt with by G. Rawlinson, in *Ancient Monarchies*, Vol. III, 490 ff. He sums it up thus: "On the one hand, the impostor had to guard against acting in any way which would throw suspicion on his being really Smerdis, the son of Cyrus. On the other, he had to satisfy the Magian priests, to whom he was well known, and on whom he mainly depended for support, if his imposture should be detected."

<sup>48</sup> For a thorough and competent treatment of the Persian concept of the Regnum Dei, see A. F. Von Gall, *Beitrag zur Theol.*, cap. iv: "Die Reich-Gottes-Hoffnung im Parsismus."

<sup>49</sup> *Zamyâd Yast*, XII.

<sup>50</sup> See Prof. Buckler's article, "Firdaus's *Shâhnâmâh* and the Genealogia

represented by the *Zamyad Yast*, irrespective of any theory of its actual reduction to writing,<sup>20</sup> dates from the same period as the writing of Deutero-Isaiah.

Moreover, Deutero-Isaiah is the sole channel through which the Cyrus heroic legend, or saga, enters Jewish thought. In fact the sole reference to the Achaemenid House in Jewish literature prior to P, results in the transformation of the Messiah-kingship of the House of David into a Messiahship of oecumenical significance and claims to be fulfilled in the triumph of the god of Jerusalem over the god of Babylon.<sup>21</sup>

The conflict between Jerusalem and Babylon as rival claimants to be the theocratic centre of the earth arises long before the period under discussion, and from the Jewish side at least the element of hostile rivalry is clearly attested. Burrowes<sup>22</sup> has worked out the rise and development of this conflict, and it need not detain us here.

But the issue between Jerusalem and Babylon for hierocentric supremacy has its parallel in the similar counterclaim against Babylon made by the capital of Media, Persia, or Elam (it is impossible to ascertain which, and immaterial) reflected in a passing allusion in the Zand Avesta in opposition to Bawri (Babylon).<sup>23</sup> This brings us to the issue at stake. Can it be shown that Deutero-Isaiah was applying the technique of Elamite or Zoroastrian theories of kingship<sup>24</sup> in order to transfer to Yahweh the triumph

Regni Dei," *JAOS*, Supp. no. 1, Sept. 1935, pp. 1-21. In this essay is traced the history of the philosophy of kingship which begins with the Cyrus "royal shepherd" saga and eventually passes outside the Persian realm to penetrate virtually the whole of Asia Minor.

<sup>20</sup> F. Nau, "Étude Historique sur la Transmission de l'Avesta," (*Rev. de l'Hist. des Rel.*, XCV, 1927), pp. 149-199 for a full discussion. Nau places the reduction to writing after the death of Artabanus Parwiz. It may be suggested that the Avestan antagonism to Alexander arises from his identification with Babylon and is a continuation of the Medo-Babylonian feud.

<sup>21</sup> See Ruckler, *op. cit.*, n. 9, p. 9 ff. Also, his paper, "Regnum et Ecclesia," *Church History*, Vol. III, no. 1, March, 1934, pp. 24-29.

<sup>22</sup> See above, n. 6.

<sup>23</sup> S. B. E., Vol. XXIII: *Ahan Yast*, VIII, 28-29 (p. 60); notice the editor's footnote on this passage.

<sup>24</sup> Note esp. IV: 10a: *לֹא יָשׁוּב דְּבָרִי וְלֹא יָשׁוּב*. "There has gone out from my mouth a truth; a word and it shall not return."

This concept, i. e. of the irrevocable nature of a divine law or word, is generally regarded as being peculiar to the Medo-Persian religion—and it is so (*Ch. Dan.* VI: 16, *ve the law of the Medes and Persians*, "that no in-

in the struggle of the gods (*Götterkampf*) which others assigned, and erroneously in his eyes, to Mithra, Ahura Mazda, Sin,<sup>26</sup> Marduk and Nebu? There are at least two positive lines of evidence (a third, more conjectural, is taken up in note 28), which lead toward the establishment of this position. The first is that Elam and Israel were both vassal states under the neo-Babylonian dynasty,<sup>26</sup> and as fellow-subjects resident in the capital had, consequently, a common political grievance that would naturally draw them together. Secondly, they had both, as we have seen, a religious animus<sup>27</sup> against Babylon, which, under the circumstances of their common hatred of idolatry, would constitute an additional bond.

These two certain facts, namely the political and religious antipathies toward Babylon which Elam and Israel held in common, with the support of the problematical but, to my mind, not improbable etymological link of Cyrus' name (which is taken up in detail in note 28 *q. v.*),<sup>28</sup> suggest that the prophet was in direct con-

terdict nor statute which the king establisheth can be changed"; also Herodotus, XXXI, 9-10, and Esther i: 10 ff.). The possibility of Persian thought in Deutero-Isaiah in this concept appears, then, not wholly improbable.

<sup>26</sup> Dougherty, *op. cit.*, n: 4. In the paragraphs pp. 153-157 he deals with the singular devotion of Nabonidus to the Sin worship.

<sup>27</sup> It is noteworthy in this connection that Cyrus found Elam a willing and useful ally when finally he attacked Babylon. Dr. R. Campbell Thompson, in his essay in the *Comb. Ann. Hist.* (Vol. III, ch. X, p. 223) summarizes the military data at hand.

<sup>28</sup> Primitive Mithraism is as conspicuously free from idolatry as is primitive Zoroastrianism. This fact would render the Babylonian idolatry as odious to the Elamites as it was to the Jews, whose abhorrence is so forcefully reflected by Deutero-Isaiah. Herodotus (I, 131) is struck by the Mithraic antipathy towards idolatry.

Jastrow (*op. cit.*, n. 6) sees in this fact an explanation, in part at least, of Cyrus' sympathy for the Jews. He states more specifically that this sympathy was partially due "to the similarity of the conceptions of divine government in Zoroastrianism and post-exilic Judaism; the attitude of such prophets as the second Isaiah towards Cyrus was not prompted solely by the favour shown the Jews by Cyrus, but arose in part from the bond of sympathy between Zoroastrianism and monotheistic Judaism" (p. 61, note 1).

<sup>29</sup> The data here taken up are concerned with the etymology of the name "Cyrus" in its bearing upon our discussion. Upon this evidence must rest our answer to the question of whether or not Deutero-Isaiah was acquainted with the language of Elam, and the extent of that acquaintance.



tact with Medo-Persian thought. However different in some respects the Jewish and the Zoroastrian religious and political traditions were, they were forced for the moment into a position of alliance against a common foe to which they were both subject.

M. Huart in his *La Perse Antique et la Civilisation Iranienne* makes this assertion: "Les Perses disent que ce nom (Kūrus) signifiait, 'soleil,' et le rapprochaient, par conséquent, du *heere* zend; mais ce n'est peut-être qu'une étymologie populaire; et ce nom est anzanite, il signifiait, 'berger.'" (p. 41, note 1). So then, the key-words in this discussion are "sunlight" (*hwarand*) and "shepherd." The etymologies of these two words respectively are taken up in the paragraphs immediately following:

1. For a full review of the etymology of *hwarand* (הַוְרַיִנִי) see W. Geiger and E. Kuhn, *Grundr. Jr. Phil.* i: 37, 95, 149, 180, 209, 282 (cf. הַוְרַיִנִי, 267; ii: 77, 387, 423; also see O. G. Von Wessendonk, *Das Welt bild der Iranier*, pp. 19, 47, 77, 80, 81, 133, 143, and note.

2. "Shepherd" רֹעֶה. This word may come from either of two verbal roots in Hebrew, both רָעָה, one meaning "to pasture, tend, graze," and the other, "to associate with." Hence only the pointing remains to distinguish between רֹעֶה "my shepherd," and רֵעִי "my friend." It is true that the Massoretic text has only the former, and it is this which we should prefer to accept. But the possibility of a double entendre in רֵעִי as (1) the friend of God, the king, and (2) the shepherd of God, also a king, is not out of the question. But when Deutero-Isaiah speaks of Abraham as the "friend of God" (xli, 8), he uses, not the root רָעָה, but אָהַב. That the title as applied to Abraham is a royal title is evident from Gen. xli: 2-3, xvii, 3-4, 7; xviii: 17; xx: 16-17, etc; and II Chron. xx: 7. It is plain from Gen. xiv: 22 that Abraham has declared his kingship as over against that of the king of Sodom, and it is noteworthy that this appears in a Melchizedek passage.

But the translation "herdsman" or "shepherd" is favored by both text and tradition. And if this is accepted, then the following conclusion is in order: since Herodotus (I, 113), in relating Cyrus' rescue by a herdsman after being exposed as an infant to die according to the order of Astyages, also relates that the herdsman changed Cyrus' name. The purpose of this step must have been to prevent Astyages' learning that the child still lived. It was the royal implication of the name Cyrus which, according to this suggestion, made it dangerous for the child to retain.

Another possibility is that the change of name reflects a rationalization of the name Cyrus (if M. Huart's statement is correct), an obvious disguise to hide the name by which he was known at the Median court. The two reasons I have suggested might well have been combined in the step. At any rate, they make practically certain one fact: that the name Cyrus is connected with the idea of shepherd.

The most notable "shepherd" passages in Deutero-Isaiah are: xl: 16; xlii: 2-3, 14; xlii: 1-5; xlv: 14 ff. It is quite legitimate to include in this category any passage where the shepherd theme is dominant and the meta-

The importance of the possibility suggested in note 28, that Deutero-Isaiah was sufficiently familiar with the Elamites to be acquainted with their language, lies in the fact that it makes reasonable this conclusion: that, having some knowledge of their idiom, he knew something of their political and religious outlook. If this is the case, then the words which the prophet ascribes to Yahwēh, "Cyrus my shepherd," may be taken as an assertion by Yahwēh that the Anshan shepherd is not Mithra's shepherd, but my shepherd.<sup>25</sup> This is but one form in Deutero-Isaiah's "conveyance" of authority from the "gods of the nations" to Yahwēh. It is perhaps the most striking case, but it is not alone. Associated with it is the whole conception of the Glory (הִדְבָּר הַנִּיבֵּן) which can be identified with the *hvarənd kavəram* of the Avesta, and with which it corresponds in every detail.<sup>26</sup> In both the Zoroastrian tradition—

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pher obvious, even though the term רֹעֶה is not actually used. Throughout caps. xl-xlviii the "shepherd-motif" provides the metaphor on which the whole structure of the Regnum Dei is based. In xli: 11, it is said of Yahwēh that "he shall feed his flock like a shepherd (רֹעֶה)." Then—most significantly—the term is applied (xlv: 23) to Cyrus, and the prophet, on behalf of Yahwēh, speaks of him as מֶלֶךְ רֹעֶה—"Cyrus, my shepherd" (For meaning of רֹעֶה, see above; cf. nrs. 5, 12 above). The real significance of this passage can be grasped only if the full force of the pronominal suffix "my" is recognized. The first fact that emerges is that, since the role of Yahwēh is that of a divine shepherd, then "his chosen" human ruler, who will execute his divine role upon earth, is likewise a shepherd. But a second fact also presents itself as a considerable possibility. The idea of the "shepherd-king" belongs to the Davidic tradition in Israel. This is a natural outcome of the story of David's own life. It is pertinent to ask, then, if Deutero-Isaiah, knowing the real meaning of Cyrus' name, was not further influenced by this knowledge to declare Cyrus to be the "anointed one" of Yahwēh? And may not the prominence of the "shepherd" passages (listed earlier in this note) in his writing be likewise attributed to the prophet's knowledge of the dialect meaning of Kurash?

<sup>25</sup> Cf. מֶלֶךְ רֹעֶה of the text, and Duhm's comment, above, n. 12.

<sup>26</sup> The three parallel ideas, in Hebrew and Avestan respectively, which establish this identification, are: (a) the material manifestations of the Glory; (b) the king as shepherd; (c) the royal righteousness. Refs. for each of these in the Zangbad Yast are as follows:

(a) On the material manifestations of the Glory. II, 11-12: "So that they [the good creatures] may restore the world, which will (thenceforth) never grow old and never die, never decaying and never rotting, ever living and ever increasing, and master of its wish, when the dead will rise, and

pre-eminently in the *Zamyād Yast*—and in the Hebrew, the *hwarand* or the כְּבוֹד is a divine gift. To use the Avestan phrase, it “cannot forcibly be seized”; otherwise, it eludes its pursuer. Whether the Hebrew word used is כְּבוֹד, as in Deutero-Isaiah and the P passages, or כָּרָה, as in Daniel, the result is the same: the Glory is the elusive possession of the king, whereby the king is the king, the representative of God on earth, and the incarnation of divine majesty. It is to the king as the temple<sup>21</sup> (I use the word advisedly) of the divine Glory that *siydaš* is performed to the king. And the only question to be decided is, whence cometh the Glory? Who is the true god, whose Glory is imperishable?

Deutero-Isaiah's answer, which is his central theme, is that the true Glory is the כְּבוֹד ה', as over against that of the gods who give *their* glory unto “graven images” (xlii: 8). It is on the point of the material manifestation of the divine Glory that Deutero-Isaiah and the *Zamyād Yast* correspond most strikingly. In cap. xl, the prophet draws a radiant picture of the world upon which the כְּבוֹד ה' shall be revealed: “Every valley shall be exalted, and every mountain and hill shall be made low. . . . And all flesh shall see

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immortality will come, and the world will be restored at its wish; when the creation will grow deathless,—the prosperous creation of the Good Spirit,—and the Drug shall perish, though she may rush on every side to kill the holy beings; she and her hundred-fold brood shall perish, as is the will of the Lord.” Also: IV, 23-24; VII, 22-23; X, 47-60; XI, 72; XII, 75-76; XIII, 79-80; XIV, 84-87; XV.

(b) *On the king as shepherd.* VII, 31, 32: “That [the kingly Glory] came unto the bright Yima, the good shepherd, for a long time, while he ruled over the seven Karshvaras of the earth, over the Daēvas and men, the Yatus and Pairikas, the oppressors, the blind, and the deaf. . . . But when he began to find delight in words of falsehood and untruth, the Glory was seen to flee away from him in the shape of a bird. When his Glory had disappeared, then the great Yima Khashāta, the good shepherd, trembled and was in sorrow before his foes; he was confounded, and laid him down on the ground.” Also: VIII, 54 (cf. esp. Isaiah xli: 2-3).

(c) *On the royal righteousness.* XII, 74 (quoted in text, below, p. 167). Cf. Buckler, *op. cit.*, n. 19; p. 18, n. 54.

<sup>21</sup>—wherein the כְּבוֹד becomes מִשְׁכָּן—dwelling-place, or “tabernacle.” The following O. T. passages point clearly to the divine or at least sacrosanct nature of kingship in Israel: I Sam. ix: 15-17; x: 1, 17-25; xiii: 11-14; xv: 10-28; xvi: 13-14; xxiv: 48; xvi: 6-12; Ex. xx: 27; Ps. ii: 12. See article by J. M. P. Smith, *AJSL*, Vol. XXXIX, pp. 32-39, “Traces of Emperor Worship in the Old Testament.”

it (the revealed Glory) together." This vision has its counterpart in the *Zamyād Yast*, where it is said of the "bright Yima" that, during his possession of the Glory, "there was neither cold wind nor hot wind, neither old age nor death, nor envy made by the Daēvas . . ." (VII, 33).<sup>82</sup>

In both Deutero-Isaiah and the *Zamyād Yast*, peace and prosperity follow from the possession, or revelation, of the Glory. In fact, to use ordinary religious terminology, these are the "gifts" of the Glory. And the principal of these are צְדָקָה in Hebrew, and *Ekshathra vairya* in the Avestan tradition.<sup>83</sup> The two words are, for practical purposes, synonymous, and their parallel use in Deutero-Isaiah and the *Zamyād Yast* definitely establishes them as royal attributes. One example must suffice: in xlii: 6, Deutero-Isaiah says of the "righteous servant," Israel, אֲנִי יְהוָה קָרָאתִיךָ בְּצֶדֶק "I am Yahwēh, I have called thee in צְדָקָה," which may be rendered either "righteousness" or "victory." In the *Zamyād Yast* (XII, 74), there is mentioned the "kingly Glory . . . that gave unto Kavi Husravah for the well-shapen strength, for the righteousness of the law, for the innocence of the law, for the unconquerable power of the law." Notice in both cases the near-identity of the ideas of "righteousness" and "victory" when applied to the possessor of the Glory. I suggest that there is a parallel "genealogy" here: the *hvarəno karasm*, or קְרָסְמִי יְהוָה, produces *vairya*,<sup>84</sup> or צְדָקָה; and the consequence is victory. In the *Zamyād Yast* the sequence unmistakably follows this order. And in Deutero-Isaiah, the passage under consideration (xlii: 6-7) very strongly implies the same sequence: "I Yahwēh have called thee [Cyrus]<sup>85</sup> in

<sup>82</sup> For other allusions in the *Zamyād Yast* to the material manifestations of the Glory, see above, n. 30a.

<sup>83</sup> For the relation between *Ekshathra* and *hvarəno*, and the identity between the Avestan *hvarəno* and the Hebrew *keboḏ Yahu* in post-exilic Israel, see Von Gall, *op. cit.*, n. 17, pp. 230-238. In this discussion the writer evinces proof that, after Deutero-Isaiah, the term *keboḏ Yahu* becomes a technical term in Israel analogous to the *hvarəno*, "anscheinend auch unter dem Einfluss der persischen Theologie."

<sup>84</sup> See above, *ref.* n. 33.

<sup>85</sup> I have interpolated the name of Cyrus here advisedly. This is admittedly a "Suffering Servant" passage, and its direct reference is to Israel and not to Cyrus. But it seems very probable here that the heroic character, of reputation, of Cyrus, so much in the minds of his contemporaries

פִּלְיָ, and will hold thy hand, and will keep thee, and give thee for a covenant of the people, for a light of the gentiles; to open the blind eyes, to bring out the prisoners from the prison, and them that sit in darkness from the prison-house." Even as the king earns his title among men by virtue of his victory, so his righteousness, which is vitally connected with victory, bears witness to his possession of the divine Glory whereby the victory is achieved.

The third and last point in our assessment of Deutero-Isaiah's "conveyance" of divine authority through the agency of Cyrus from Ahura Mazda to Yahwēh, is summed up in xlv: 7:

"I am Yahwēh, and none else.  
Fashioner of light and creator of darkness,  
Maker of well-being and creator of ill:  
I, Yahwēh, am maker of all these."

This is the prophet's final argument that Yahwēh is victorious over Ahura Mazda in the Götterkampf, and his יְיָ supersedes the *hcrenō kavem* "made by Mazda." For Ahura Mazda is the god only of light and peace and good. Ahrimān is the god of darkness and evil—*ergo*, since Yahwēh is the Creator of both light and darkness, he alone, not Ahura Mazda and Ahrimān, who are his creatures, can claim supreme divinity. This point is emphasized in xlv: 9-12, in which is developed the supremacy of the Creator over the creature. The whole passage constitutes a unity of argument that cannot conceivably have any other end in view.

We are now in a position to turn to Deutero-Isaiah's historical treatment of Cyrus. It is a Cyrus not yet in possession. The whole passage (caps. xl-xlviii) is prophetic in the sense that it is speculative concerning the policy which will result from the approach of Cyrus and his certain capture of the city. Notice first of all that the Götterkampf between Yahwēh and the gods of Baby-

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including Deutero-Isaiah, forms the archetype or pattern of the prophet's conception of the glorified Servant, Israel. The poetic allusion in this passage to him who is "to bring out the prisoners from the prison, and them that sit in darkness from the prison-house," is too strongly suggestive of Cyrus' release of the people in bondage to Babylon to be ignored or explained as accidental similarity. Moreover, if once we grasp the force of the elusiveness of the Glory before attempts of forcible seizure, the transition from military triumph to complete reconciliation and suffering follows directly.

Ion and Persia has its parallel-motif, if not its actual suggestion, in the internecine struggle among the gods of Babylon. The conflict is most acutely seen in the devotion of Nabonidus to Sîn, the moon god, apparently in preference to the sun gods Bel and Nebo.<sup>56</sup> Though the Marduk priesthood acknowledge the other gods in so far as they are vassals to their gods, the action of Nabonidus in giving his supreme devotion to one of these subordinate, and subject deities constitutes, in their eyes, a definite usurpation of authority.

Secondly, Deutero-Isaiah is in a position to see a combination of two facts that leads him to the conclusion upon which his whole "Cyrus-theme" is based: first the gods of Babylon are bankrupt and their power dissipated in the *Götterkampf* among themselves<sup>57</sup> that marks their doom; secondly, Cyrus' conquest of Babylon is inevitable. And this situation gives the prophet the opportunity, and the inspiration, to mark down the supremacy of the god of whose Glory Cyrus is the possessor. It cannot be Marduk, because Marduk's authority is crumbling and his idols are soon to be cast out (xlv: 1);<sup>58</sup> it cannot be Ahura Mazda, because he is a created being. So Deutero-Isaiah seizes this opportunity to declare that, whether Cyrus knows it or not (xlv: 4), that god is Yahwêh, who for the sake of his servant Jacob and Israel his chosen one, has called him by name "Cyrus, my shepherd."

#### ADDITIONAL NOTE

There has been no place in this paper where the evidence from comparative eschatology relative to Medo-Persian influences upon Deutero-Isaiah could relevantly be taken up. I have had to limit my attention to but one aspect of the whole question, i. e., that which concerns Deutero-

<sup>56</sup> Dougherty, *op. cit.*, n. 4, p. 154-155.

<sup>57</sup> Dougherty, *op. cit.* above, pp. 150-7 for comment on the priestly opposition to Nabonidus' religious program.

<sup>58</sup> The prophet actually ridicules their helpless estate (xlv: 1-2). They (i. e., the idols of Bel and Nebo), are unable even to walk, but must be loaded on the backs of beasts and cattle. It is Deutero-Isaiah's teaching (xlv: 7) that the gods whose deity resides in idols are as dead as the idols themselves. So Bel and Nebo, who live only in their idols (which are proved to be dead by their inability to defend themselves), go into exile. The conflict between Israel and Babylon is ended, because (xlv: 3-7) Israel can call upon Yahwêh, but Bel Marduk, who has claimed to be the supreme God, is no god, but merely a fabrication of idolaters.

Isaiah's doctrine of the "conveyance" of divine authority, through the agency of Cyrus, from the gods of Babylon and Persia to Yahweh. But the eschatological data are too important to be left out of the consideration. For a thorough analysis of this field of evidence--see A. F. Von Gall's work, *Basileia tou Theou*, especially the section on Deutero-Isaiah, pp. 173-188. He reaches this conclusion:

"Fast möchten wir denken, Deutero-Jesaja habe vom Sakjanti der persischen Religion gehört und diesen Begriff auf seinen Gott übertragen. Und dann sieht uns dieser Kyros mit seiner weltgeschichtlichen Aufgabe, die ihm der Dichter zuweist, wieder als der Sieger Astarotereta aus; er ist wie Zarathustra Gottes Hirde, er soll ja auch die Völker zur Erkenntnis des Einen Gottes führen. Jahwes Geist, der ein Neues schafft, erinnert uns an Ahura Mazda's heiligen Geist . . . Und schliesslich findet, wenn wir bei Deutero-Jesaja den Einfluss persischer Ideen annehmen, vielleicht auch das Geheimnis des unschuldig leidenden frommen Gottes Knechtes, der seine Auferstehung erlebt, seine Lösung."



## THE ATTITUDE OF HAN KAO-TSU TO CONFUCIANISM

HOMER H. DUBS

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THE ACCESSION of Han Kao-tsu, the founder of the Han dynasty (202-195 B. C.), marks the victory of the Confucian conception that government is for the benefit of the people and should be founded upon justice, over the legalistic conception of arbitrary and absolute sovereignty. While Kao-tsu and his successors technically remained absolute sovereigns, in practice their powers were much limited by custom.

The theory and practice of government in the Ch'in state and empire was that of centralized absolutism. The Ch'in ideal of government was that "none will dare not to do what the ruler likes, but all will avoid what he dislikes."<sup>1</sup> The primary concern of Lord Shang's theorizing, like that of Machiavelli, was to make the ruler all powerful. In this respect, the First Emperor of the Ch'in dynasty was a thorough-going exemplification of the legalist theory.

While Kao-tsu adopted many of the Ch'in practices, he nevertheless realized that what the people most condemned in the Ch'in rule was precisely this unreasoning absolutism, and he carefully avoided any semblance of such absolutism. He realized that he was handicapped by his peasant birth, and knew that he must gain the good-will of the people in order to maintain his rule. Hence he consciously adopted the policy of always considering the interests of the people and the requirements of justice and righteousness. Before he entered the state of Ch'in as an invader, he sent an emissary to its people in order to acquaint them with his virtuous intentions. At the surrender of the Ch'in king, he was careful to be generous and indulgent and to avoid plundering the people. One of his first official acts in Ch'in was to summon the people and inform them that he was doing away with the severe and cruel laws of Ch'in—an act which helped him greatly when he had later to reconquer the region. He refused to exact food from the people for his army, preferring to use that stored up in the government

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<sup>1</sup> Duyvendak, *The Book of Lord Shang*, p. 202.



granaries. When Hsiang Yü gave him a kingdom in Han, he claimed that injustice had been done because a covenant had been broken. He exempted from taxes those people who had been too heavily burdened in furnishing the armies with supplies, and granted his soldiers various and increasing exemptions. He continued the practice of giving the representatives of the people the position of *San-ko*, and had them advise the officials so that the people would have a direct voice in government. He granted general amnesties on all appropriate occasions. He had his soldiers who had died in battle enshrouded and encoffined and sent home to be buried at official expense. He appointed caretakers for the graves of the great kings, in order that their hungry manes might not disturb the country. He waited to assume the title of Emperor until it was formally offered him by his followers, and then accepted it because "the vassal kings would be favored by it and they considered it to be an advantage to all the people in the world." At his accession he freed all slaves and restored to civil rights all refugees and exiles. He granted aristocratic ranks to all his soldiers. He fixed the amount of the military tax so that the people would not be oppressed by exactions.

More important still, soon after his accession he adopted the practice of the not taking the initiative in appointing any of his relatives or sons to any kingdoms or nobilities, but acting only at the suggestion of his followers. Of course it was always possible to give hints to others about what the Emperor wanted to be done. Yet this practice, that the Emperor should act only at the suggestion of others, became a real check upon absolutism. At first it seems to have been confined to the enfeoffment of the emperor's sons, but later it was extended to most important matters, so that the standard practice in exacting an administrative measure came to be that some official or group would memorialize the Emperor concerning what they thought should be done, and the Emperor approved the suggestion.

The custom that the ruler act at the suggestion of his important subordinates was a real and often effective limitation upon the imperial power. When the Emperor Hsiao-hui died, the Empress Dowager née Lü was unable to obtain any effective power until one of the great officials suggested to her that she appoint her two nephews to the highest positions in the government and other members of her own family as kings. Until that suggestion was

made, she could only spend her time weeping helplessly. After it had been made, she rewarded very highly the person who first suggested it. When custom was disregarded by the ruler, the results were disastrous. After the Empress Dowager née Lǐ had dismissed the son of Emperor Hui from the throne, she asked the high officials to suggest his successor. The Emperor Hui had had only this one son, although the Empress Dowager had enclosed six other babies on the pretense that they were his sons. The officials refused to suggest any of them for the throne, and the Empress Dowager, on her own motion, appointed one of these babies as Emperor. But the officials, by refusing to suggest him, had disclaimed responsibility for him, and, when the Empress Dowager died, they selected a son of Kao-tsu as the new emperor and killed the boy whom the Empress Dowager had put on the throne.

This Han custom was expressed most forcibly after the death of the Empress Dowager née Lǐ. The high officials sent someone to tell her nephew, Lǐ Lu, who was then in control of the army, that "the establishing of the kings . . . was a matter all done after discussion with the great officials, announcement, and information to the vassal kings. The vassal kings considered it suitable." Lǐ Lu was warned that if he tried to do anything contrary to the will of the great officials, the greatest disaster would come upon him. So strongly did he realize the truth of the assumption behind those words, namely, that the rule of the emperor is not absolute but is vested in him in consultation with the great officials, that he finally (though too late) resigned his powers. After the extermination of the Lǐ family, the next emperor was chosen by the high officials and the heads of the imperial family.

The Han rulers also recognized the principle that the empire belonged, not to Kao-tsu alone, but to his followers and associates as well, for they had helped him to conquer it. As long as any of Kao-tsu's companions were alive, they and no others were given the important positions in the government. Perhaps this was the reason that the high officials tolerated the criminal Empress Dowager née Lǐ as long as she was alive. She had taken an active part in the conquest of the country. In 179, the Emperor Wen gave additional rewards to those of Kao-tsu's followers who were still alive, and sought out some thirty of his followers who had not been previously rewarded. For this reason, until 176, the imperial chancellors, 相 were all military men. Not until 150 was there

an imperial chancellor who had not been a follower of Kao-tsu, and then it was Chou Ya-fu, the son of Kao-tsu's General Chou P'ao, who had also been chancellor under Emperor Wen. The first chancellor who was not even a son of Kao-tsu's followers, was Wei Wan, appointed in 143 B. C. Thus Kao-tsu's followers controlled the government for sixty years after his coronation. Even after that time, the government made an effort to continue the marquises of Kao-tsu's outstanding followers, in spite of the lack of heirs and their derelictions. As Kao-tsu said in an edict of 196 B. C., "I, by the spiritual power of Heaven and by my capable gentlemen and high officials, have subjugated and possess the empire . . . Capable men have already shared with me in its pacification. Should it be that any capable persons are not to share with me in its comfort and its profit?" The Emperor was thus limited by the necessity of giving high office to those who did outstanding services to the state.

Since the government cultivated popular support and the Emperor recognized that he depended upon his officials, it was quite natural that Kao-tsu should have initiated the procedure which finally brought about the Chinese civil service examination system. In an edict of 196 B. C., possibly at the instigation of Hsiao Ho, Kao-tsu ordered the officials to send to the Chancellor of State all people of excellent reputation and manifest virtue, so that their accomplishments and appearance could be recorded. The Emperors Wen and Wu continued this practice, and the examination system gradually grew out of it.

We have said that this conception of imperial rule as limited by consultation with the high ministers and by moral considerations was specifically Confucian. This doctrine is to be found in the *Book of History*, where the great rulers consult their ministers on important matters. It is the outgrowth of the attitude represented in that *Book* (II, VII, iv, 7), that Heaven sees as the people see, in *Mencius* (VII, II, xiv, 1), when he says that the sovereign is inferior to the people and the spirits, and in *Hsün-tzu* (IX, 4), "The prince is the boat; the common people are the water. The water can support the boat, or the water can capsize the boat." The Han dynasty became the first great patrons of Confucianism. It has not always been realized that this Confucian influence began with Kao-tsu.

Kao-tsu was not himself a Confucian. He seems indeed to have

had, especially in his earlier days, a deep dislike for the learned pedants of the time. It is said, in the biography of Li Yi-chi, that before 307 n. c., probably when Kao-tsu had just started out as a general, some literati came to him in full costume, with their literati's bonnets on, and that Kao-tsu, in order to show his contempt for them, suddenly snatched off a bonnet and urinated into it. It is also told that when, in May 205, Shu-sun T'ung came to Kao-tsu and wore his scholar's gown, Kao-tsu hated it, so that Shu-sun T'ung changed and wore short clothes like the people of Ch'ü. Thus Kao-tsu had an aversion to the sight of the Confucian literati.

That fact, however, does not warrant us in holding that Kao-tsu disliked Confucianism and was not influenced by it. Quite the contrary seems to have been the case. In Kao-tsu's father's home, four sons grew to maturity. The two oldest sons seem to have farmers; Liu Chi (Kao-tsu), the third to grow up, studied military matters and became the chief of a *ling*; Liu Chiao, the youngest, was sent to the state of Lu, which was not far from the homestead at P'ei, and studied with three Confucian teachers. Later he studied the *Book of Odes* with Fou-ch'ü Po, a disciple of Hsün-tzu, who became the most outstanding member of the Confucian school. After Liu Chiao had been later made the King of Ch'ü, he summoned the three Confucian teachers with whom he had studied in his youth, and honored them as his Palace Grandees. In the time of the Empress Dowager *née* Lü, he sent his own son to study under Fou-ch'ü Po. Liu Chiao is furthermore said to have been very fond of the *Book of Odes* and to have written a commentary on it. Thus Liu Chiao, the younger brother of Kao-tsu, was himself a devoted and life-long Confucian, who secured an excellent Confucian classical education at the center of Confucian culture.

Liu Chiao was an intimate follower and companion of Kao-tsu from the time that the latter started out as a general. Kao-tsu left his older brother, Liu Chung (the oldest, Liu Po, had died) and Shen Yi-chi at the homestead to care for his father and wife, and took his other friends and followers with him to swell his army. It is said specifically that when Kao-tsu became emperor, Liu Chiao waited upon him. He and Lu Wan, a boyhood friend, were the two persons closest to the Emperor. They had access to his private chambers, served as intermediaries, carried messages, and helped him to decide matters and make secret plans. Through his

brother, much Confucian influence undoubtedly reached Kao-tsu. No one else of any education had as close relations with him; while Kao-tsu disliked the pedant and the pedant's appearance, yet he probably welcomed the Confucian teaching when it came to him divorced from pedantry.

There were several others who undoubtedly influenced Kao-tsu towards Confucianism. The earliest was Chang Liang, who came to Kao-tsu in February 208 B. C. He was not a literatus, but a politician, the descendant of the chancellors in Han. He was a well-educated man, and on occasion is represented as using classical allusions in a thoroughly Confucian manner to back up his advice on politics. Kao-tsu respected him highly, and publicly recognized him as his best adviser.

Li Yi-chi was a well-read Confucian who came to Kao-tsu in March / April 207. He was known to the people of his town as a Master or teacher 生, and came voluntarily to call upon and advise Kao-tsu. The latter received him contemptuously, squatting upon the *k'ang* with two maids washing his feet. But Li Yi-chi was more than a pedant, even though he probably wore his scholar's robes on this occasion. He was over sixty, six feet tall (English measure), and fearless. He reproved Kao-tsu for his discourtesy; the latter, who seems to have been trained to respect his elders, was impressed by the old man, arose, dismissed the maids, begged Li Yi-chi's pardon, and escorted him to the seat of honor. At that time Kao-tsu could not afford to ignore any worthwhile advice; Li Yi-chi delighted him with stories of earlier times, then gave him the directions necessary for capturing a neighboring city. For that Kao-tsu rewarded him; the old man was quite garrulous; he had earned the nickname of "the Mad Master," and Kao-tsu liked a practical joke. At the time, Kao-tsu was giving honorary titles to those of his followers who distinguished themselves; to Li Yi-chi he gave the title of "Laird of the Wild Wastes, with the income of [lit. 'eating'] his words"! Nevertheless Kao-tsu respected the old man, consulted with him about important matters, and sent him as a confidential envoy on important commissions.

In April 205, when Kao-tsu came to Lo-yang, the old gentleman Tung, who was a *San-lao* or leader of the people, stopped him and advised him, in thoroughly Confucian terms, to declare a crusade against Hsiang Yü because the latter had caused the assassination of his superior, the Emperor Yi. This practice of leading a

military force to chastise a wicked ruler, was typically Confucian; in the *Book of History*, Kings Tang and Wu are both said to have led such a crusade and to have founded their dynasties in so doing. The notion was welcomed by Kao-tsu: he found it worked, for it enabled him to lead a coalition army of 560,000 men with five kings against Hsiang Yü, and to capture his capital. After this experience, Kao-tsu would not likely have looked with disfavor upon a teaching that so helped him against his enemy. Confucianism now became to him a most useful and helpful philosophy.

Shu-sun T'ung had been made a *Po-shih* by the Second Emperor, and had been with Hsiang Yü in that capacity. When, in May 205, Kao-tsu captured P'eng-ch'eng, Hsiang Yü's capital, Shu-sun T'ung, who followed the policy of making himself useful to whoever was in power, surrendered to Kao-tsu. He pleased Kao-tsu with stories of fighting and war, avoiding any typically Confucian teaching. Kao-tsu made him a *Po-shih* and gave him a title. When Kao-tsu ascended the throne, Shu-sun T'ung arranged the ceremony.

After the court had been established, Kao-tsu found himself at a loss without any court ceremonial. He himself believed in simple direct intercourse without bothering about ceremonial. Possibly what he most disliked in Confucianism was its excessive ceremonialism. Now Kao-tsu's courtiers, who were his old army companions, behaved in the court just as they did in camp. Especially when under the influence of liquor, they quarrelled, shouted, acted without manners, and even pulled out their swords and hacked at the columns of the palace. Kao-tsu was very much worried, for he saw that this sort of conduct must somehow be stopped. Shu-sun T'ung offered to remedy the matter by arranging a court ceremonial. Kao-tsu saw that something of that sort was necessary, so told him to go ahead, with the admonition, "Make it easy." Shu-sun T'ung called some thirty odd literati from Lu, and with them created a court ceremonial by mixing the Confucian ceremonial with that of the Ch'in court. After more than a month of preparation, the ceremonial was performed out in the country before Kao-tsu, who approved it, and had it put into practice at the court of November 201. After the ceremony, Kao-tsu was so impressed that he said, "Now, I have today learned what is the greatness of being an emperor." Thus Kao-tsu even accepted a semi-Confucian ceremonial for his court.

Lu Chia came to Kao-tsu possibly at the same time as Li Yi-chi, for we find them associated together only a few months later. He was also a highly educated man and was sent as an envoy to Ch'ao T'o, King of Nan-yüeh, whose capital was at the present Canton. After his return in 196 or 195 B. C., he is said to have quoted the *Book of Odes* and the *Book of History* to Kao-tsu, whereat the latter scolded him and said, "I got the empire on horseback; why should I bother with the *Book of Odes* or the *Book of History*?" Lu Chia replied, "You got it on horseback, but can you rule it from horseback?" Then he proceeded to quote cases from ancient history of kings who had lost their thrones through their wickedness, concluding with the Ch'in dynasty, which Kao-tsu had himself overthrown. Kao-tsu blushed for shame and asked Lu Chia to write a book explaining why these rulers had lost their kingdoms. That book has come down to us. It is a piece of thoroughly Confucian exhortation, which argues that the rise and fall of dynasties depends on their virtue. It is said that when each chapter was completed, Lu Chia read it to Kao-tsu, who praised it and gave the book its title, the *Hsin-yü*, "*New Discourses*." This event undoubtedly deepened Kao-tsu's gradual conversion to Confucianism.

As his experience of statecraft increased and as he saw deeper into the necessities of an empire, Confucianism thus looked more and more attractive. It is recorded that when, in December / January 195/4, Kao-tsu passed thru Lu, he sacrificed a suovotaurlia to Confucius, but this record is very likely unhistorical.

The climax of Kao-tsu's allegiance to Confucianism came when he proposed to change the succession to the throne. Chang Liang, Shu-sun T'ang, and others remonstrated with Kao-tsu against this change, but without effect. Because of Kao-tsu's lack of manners, some Confucians had refused to come to his court. Kao-tsu had by this time realized how deep was the influence of the Confucians with the people. He knew that just as he had won the throne, so his successors could only keep it by securing the respect of the people. When, in the first part of 195 B. C., Kao-tsu came actually to change the succession, and found that his Empress had succeeded in bringing to follow her son Ying, the Heir-apparent, four outstanding Confucians who had previously declined to come to Kao-tsu, he refused to change the succession, for he knew how powerful

was their influence. Thus Kao-tsu finally bowed to the influence of Confucianism.

This gradual turning turning of Kao-tsu to Confucianism does not mean that other philosophies had no influence. Chang Liang was much more of a Taoist than a Confucian. Ts'ao Ts'an, Kao-tsu's best fighter, was a devotee of Lao-tsu. The imperial administration was taken over from the Ch'in court, and brought with it much Legalist influence. It was only gradually that Confucianism gained its influence. Under the Emperor Wen, there were *Po-shih* who specialized in the non-Confucian philosophers; the only Confucian *Po-shih* at his court was Chia Yi. It was not until 141 that the Emperor Wu forbade the promotion of scholars who were learned in the non-Confucian teachings. Even after that, many Legalist practices were cultivated. Thus the growing influence of Confucianism in Han times was only a gradual growth, yet it was a natural continuation of the development in Kao-tsu's own thought.





# SU — Šiglu

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IN *JAOS* 55.430 I referred to an unpublished text from the Harvard Semitic Museum proving that *SU* is the ideogram for *šiglu* in Nuzi. Here is the transcription of the text (SMN 167)<sup>1</sup>:  
<sup>1</sup>47 [manē] kaspa<sup>ms</sup> <sup>2</sup>za-ar-pu šu <sup>3</sup>bēl-iddin-na <sup>4</sup>šiglu (*SU*) ū  
 zu-uz hurāsa ša bē-aš-lu <sup>5</sup>a-pil-ku-bi mār ip-ša-ša-lu <sup>6</sup>a-na šipti<sup>ms</sup>.  
 šu il-gi <sup>7</sup>ū i-na arhi<sup>ms</sup> im-pur-ta-an-ni <sup>8</sup>ga-du šipti<sup>ms</sup>-šu <sup>9</sup>a-pil-ku-bi  
<sup>10</sup>a-na <sup>11</sup>bēl-iddin-na <sup>12</sup>ū(sic!)-ta-ar <sup>13</sup>Rev-maḥar ū-nap-ta-e mār  
 šūk-ri-ia <sup>14</sup>kanuk (*NA. KIRIB*) ū-nap-ta-e <sup>15</sup>maḥar a-gi-gi mār ar-  
 te-šup <sup>16</sup>maḥar tar-mi-til-la mār šūk-ri-til-la <sup>17</sup>maḥar ba-a-a mār me-  
 li-ia <sup>18</sup>maḥar ha-ia mār zi-ri <sup>19</sup>kanuk tar-mi-til-la <sup>20</sup>kanuk a-pil-  
 ku-bi "47 [minae] of refined silver, belonging to Bēl-Iddina,  
 (and) 4 shekels and a half of gold which was purified, Apil-Kubi,  
 son of Ipānhulu, has borrowed at interest, and in the month Im-  
 purtanni, together with its interest, Apil-Kubi shall return (them)  
 to Bēl-Iddina."

5 witnesses; 2 seals; including that of the borrower.

There is another text where the word *zūzu* follows the sign *SU*  
 (SMN 73): <sup>1</sup>38 šiglu (*SU*) <sup>2</sup>zu-uz zu-la-aš-lu <sup>3</sup>ša šiparrī<sup>ms</sup> <sup>4</sup>ša  
 škalim<sup>ms</sup> <sup>5</sup>i-na gāl <sup>6</sup>i-[ri-bi]-ilū <sup>7</sup>amū<sup>ms</sup> (*amukāri*) (*'DAM*). [*QAE*])  
<sup>8</sup>ua-ad-uz <sup>9</sup>Rev-aḥar mi-ri-bi-lu "38 shekels and a half . . . of

<sup>1</sup> SMN: Catalogue number of the Nuzi tablets in the Harvard Semitic Museum.

<sup>2</sup> The use of the plural sign *MS* in Nuzi seems to be threefold: 1) to indicate the plural after words written either ideographically or not. 2) to indicate a plural quantity (SMN 167, line 1; 73, 3) or the plural subject of a verb. 3) to indicate the final long vowel of a word; this is the only explanation of the presence of *MS* after *arhi*, and probably also of *šiptu*, because *šiptu* is never used in the plural in Nuzi texts, except in Chiera, *Joint Expedition with the Iraq Museum* 151. 12. [See, however, *Ann. Am. Sch. Or. Res.* XVI 123. E. A. S.]

<sup>3</sup> The difficulty in this line is the division of the words: it could also be divided *zu-uz-zu la-aš-lu*, or taken as a whole word [the best solution]. There are many names of objects beginning thus in several Nuzi inventory lists: *zu-uz-zu-lu* (SMN 315, 18; 435, 22 etc.). In any case, the second object has a Hurrian name and is something made of copper.

copper, belonging to the palace, into the hand of Iribi-Ilu, the merchant, were given. Seal of Iribi-Ilu."

A third text, very interesting, though unfortunately half broken is SMN 1244: <sup>1</sup>ri-bu-tum ħurāsa <sup>2</sup>na-i-ti-lu i-di<sup>3</sup> <sup>4</sup>ri-bu-tum ħurāsa <sup>5</sup>ni-iḫ-ri-ti-lu mār a-ki-te-šup KI.MIN <sup>6</sup>ri-bu-[tum ħurāsa]a <sup>7</sup>šur-ki-ti-lu KI.MIN <sup>8</sup>.... ri-b[u-tum ħurāsa <sup>9</sup>p]u-ru-ú-šu. Rest of obverse and beginning of reverse destroyed. <sup>10</sup>mišlu šihru (BAR.TUR) ħurāsu <sup>11</sup>ni-iḫ-ri-i<sup>12</sup> <sup>13</sup>mišlu šihru ħurāsu <sup>14</sup>to-ḫi-iḫ-a-pu <sup>15</sup>mišlu šihru ħurāsu <sup>16</sup>šur-ki-p-šurri mār tar-mi-ia <sup>17</sup>napḫar 2 šiglu(SU) su-uz à <sup>18</sup>UŠ-mišlu šihru ħurāsu ša nadnu<sup>19</sup>

Since about half of the tablet is lost, and the total of the present items is about half of the sum total given in line 13, one may safely guess that this refers to the whole text. However, the interest of this text is in that it gives three different weight measures for gold: the *šiglu* (shekel), the *šasu* (half-shekel) the *mišlu šihru* (the small half) and the *ribātu* (the quarter). Again here the *SU* is differentiated from the *šasu*. In addition we have two other measures, not found in any other Nuzi texts. VR 40, 51c has a *mišl šigl* but the *mišlu šihru* is so far as I know, unknown. Since we have already in this text a half shekel and a quarter shekel, there is the possibility that the *mišlu šihru* refers to the half of the fourth, i. e. the eighth.

These three texts prove beyond any possible doubt that *SU* is not the equivalent of *šasu* and is therefore the ideogram for *šiglu*. In JAOŠ 53.33 Dr. Speiser stated that "in personal communication Meissner kindly calls my attention to the fact that *SU* is part of a shekel rather than a shekel." Meissner probably referred to the fact that in Hittite inscriptions (*Boghazköi-Studien* X. 35 f.<sup>2</sup>) the sign *ZU* is an abbreviation of, rather than an ideogram for, *šasu*. But there is no indication that in Nuzi *SU* was used for *ZU* (while *ZU* is constantly used with the phonetic value *SU*). How the sign *SU* came to be the ideogram for *šiglu*, is not so difficult to understand. In Old Babylonian the sign *GIN* (the real ideogram for *šiglu*) takes a form very similar to that of the *SU*, and very often cannot be distinguished from it. Therefore it is quite possible

<sup>1</sup> The numeral 1 before *ribātu* is found only in the first line, and might have been in line 6 where it is now destroyed.

<sup>2</sup> Cf. also MDOG, I, 2, p. 10, line 195; OLZ 1918, 171 f.

that in the evolution of writing, the sign *GIN* and *SU* became the same in shape in the Nuzian script.

Two SMN texts may lead us toward a more complete solution of this problem:

SMN 494: The first two lines are destroyed. <sup>3</sup>10 MA. [NA du. e. na. <sup>mes</sup>] <sup>4</sup>a-na [x] <sup>5</sup>ba-pa-lu [subāti] <sup>6</sup>mašb(DUG.GA)-šarri il-[gi] <sup>7</sup>1 SU šī-ig-lī d[u-e-na<sup>mes</sup>] <sup>8</sup>ig-mu-i il-[gi] <sup>9</sup>1 SU šī-ig-lī KI. MIN <sup>10</sup>uš-ta-l-a il-gi <sup>11</sup>2 SU šī-ig-lī KI. MIN <sup>12</sup>Lo.E šbe-en-tum il-gi <sup>13</sup>1 SU šī-ig-lī KI. MIN <sup>14</sup>Rev. a-ga-ba-cl-lī il-gi <sup>15</sup>1 SU šī-ig-lī KI. MIN <sup>16</sup>ti-ir-wa-ni-cl-lī il-gi <sup>17</sup>du-e-na<sup>mes</sup> ša subāti(TUG. MEŠ) <sup>18</sup>[š]a dī zi-is-za ab-l[u-mi-i] <sup>19</sup>[d]u-e-na<sup>mes</sup> subāti<sup>mes</sup> (sic!) <sup>20</sup>[š]a dī gillī(MI. NI)-a-wa <sup>21</sup>[u]b-la-mi-i

SMN 808: <sup>1</sup>2 SU šī-ig-lī du-e <sup>2</sup>a-na šinā<sup>mes</sup> subāti<sup>mes</sup> šī-la-an-nu <sup>3</sup>[x SU] šī-ig-lī du-e <sup>4</sup>a-na ištēn<sup>mes</sup> subāti<sup>mes</sup> nu-hē <sup>5</sup>2 SU šī-ig-lī du-e <sup>6</sup>a-na <sup>7</sup>TRG du-ud-d[u-pu-n]a <sup>8</sup>10 SU <šī-ig-lī> du-e <sup>9</sup>a-na 1 ku-si-t[u] <sup>10</sup>Lo.E. al-ik<sup>3</sup>-[x] <sup>11</sup>Rev. a-na qāl <sup>12</sup>ba[-i-te-šup] <sup>13</sup>na-din <sup>14</sup>up-pi la-nā-zi-il-ti

In both texts the thing measured is the same. In one case it has the Hurrian plural ending -na<sup>mes</sup> together with the plural sign -mes; in the second case it is in the singular. This word is therefore a Hurrian word as is also *šuddupuna* which has also the same plural ending. All the names for garments in SMN 808, with the exception of *kustū* are Hurrian words, but they occur very frequently in other unpublished SMN texts. Nuzi garments are made either of wool or of leather. Since the Hurrian equivalent of *šēpātu* (wool) seems to be *šehtanu* (SMN 288, 8; 3587, 8), it appears very probable that *du(e)na* is the equivalent of *mašku* (leather).

The presence of the sign *SU* before the word *signu* is the clue to its meaning. The word *šī-ig-lī* is in the genitive (it could not be taken as a plural, since in SMN 494, lines 6. 8. 12. 14 we have only one *SU šī-ig-lī*). Thus the sign *SU* must represent a construct preceding a genitive. As was the rule in Nuzi, determinatives were pronounced and used in the construct case.<sup>7</sup> Therefore *SU* is here

<sup>7</sup> Cf. Fournet ZDMG N. F. I. 226.

<sup>8</sup> This is evident from the fact that the so-called determinative is very often followed by the plural sign when necessary and that sometimes the determinative is written at the end of one line, while the word itself is at the beginning of the next line: *š...a-šar anāti<sup>mes</sup> šū-ru-nū-te-e* (SMN 716).

a determinative. But why should *SU*, which elsewhere stands for *šiglu*, precede the spelling of the very word it represents? The explanation is that *SU* is really the ideogram for *masku*, leather, and as such is used as determinative before objects made of leather, like the *sianatu*.<sup>9</sup> Since, as I suggest, *du(e-na)* is the Hurrian word for leather, the scribe should have written the determinative for leather before it. But he also wanted to indicate the weight of the leather in shekels and should have used also the sign *SU*; it would have resulted in a confusion: One could have read *2 šiglu du-e-na<sup>neš</sup>* or *2 masku du-e-na<sup>neš</sup>*. Therefore, to prevent any doubt, the scribe inserted the spelling of the word *šiglu* after its ideogram.

There is however another possibility. In Cappadocian texts the ideogram precedes its spelling: *bit<sup>bi</sup>-de<sup>o</sup>*; *bit<sup>bi</sup>-tan<sup>14</sup>*. This usage is limited in Nuzi to the form *bit<sup>bi</sup>škallu* (written: *B. B. GAL. LU*)<sup>11</sup> which occurs in some wills, and where the word *škallu* takes a special meaning, not yet determined. Therefore it seems more plausible that the first alternative is the better.

<sup>9</sup> CL AJSL 51 (1934), 26 ff.

<sup>10</sup> TCL, *Lettres et Contrats*, Paris 1910, No. 240, line 5.

<sup>11</sup> Julius Lewy, *Die Allassyrischen Texte von Kültepe*, Konstantinopel, 1926, No. 40, line 1.

<sup>12</sup> SMN 2070, 12; *bit š-kal-lu rabū (ša al nu-si ilgi)*. Also: SMN 2610, 15; 2656, 8; 3559, 3.

## BRIEF COMMUNICATION

### *Buddhist Sanskrit saṃdḥā, saṃdhi(-nirmocana)*

Professor Ware's review of Lamotte's *Saṃdḥānirmocanasūtra* (JAOS 57. 122) invites Sanskritists to discuss the meaning of the difficult terms *saṃdḥi*, *saṃdḥā*. I am glad to start this discussion, since these words have interested me for some time, and I believe I have arrived at an understanding of what they mean.

Let me first point out that *saṃdḥā* is not only a verb (Ware) but also a noun; and indeed it is from the use of the noun rather than the verb that we must start. It is a synonym of *saṃdhi* as used in the title of this Sūtra. The two nouns are translated in the same ways in Tib. and Chin., and are obviously both root nouns from the same familiar Sanskrit base (one strong-grade, the other weak-grade). Of the varied renderings in Tib. and Chin. we may here ignore the meaning "joint" (whence "knot" according to Lamotte). It is one of the familiar meanings of Skt. *saṃdhi*, in both standard and Buddhist Skt., but has no bearing on the passages we are considering. If as Lamotte and Ware inform us Chinese translators sometimes use it in the title of this Sūtra, this is due to a misunderstanding, tho a not unnatural one ("unloosening of knots" seems a natural interpretation of the title of such a work!). The mistake is the more pardonable since *nirmocana* is also used in a strange sense in this title. It can only mean "expounding," literally either "releasing, letting loose" or "putting forth, emitting." Cf. *Laṅkāvetāra Sūtra* p. 5 line 5 (Ch. I, *gāthā* 10), where *deśanānayanirmuktaṃ* (*Laṅkāvetāra-sūtram*) can certainly not mean "which is not founded on any system of doctrine" (Suzuki), but rather "sent forth or expounded in or by or according to sermons and rules (of conduct)." The Mahāvīyutpatti's rendering of *saṃdḥānirmocana* by *dgoṅs-pa nas-par ḥgrēl-ba* (so Mhvy., supported by Jäschke's dictionary; Ware n. 1 quotes *ḥgrēl-pa*, supported by Das) is essentially sound, and means "explanation in true fashion of the meaning." Here *nas-par ḥgrēl-ba* (or *-pa*), "explanation as it really is," renders *nirmocana*, and *dgoṅs-pa*, "meaning," renders *saṃdḥi*. See Jäschke's dictionary under *dgoṅs-pa* II: "meaning, sense, esp. the sense of sacred words

or writings, therefore *dgoṃs-pa kgral-ba* to explain that sense, *dgoṃs-kgral* . . . commentary." Quite close to this is the Chinese translation quoted by Ware (note 6) from the Mhvy.: *kuang-k'ai chên-shih* 廣開眞實 that is, literally, "wide-opening (= *nirmocana*) true-facts (= *saṃdhi*)."<sup>1</sup> (I am indebted to my colleague Mr. George A. Kennedy for help with this and other Chinese passages.) In my opinion this is not, as Ware thinks, "a late, euphemistic, vague rendering" but comes much closer to the fundamental meaning than any of the other Chinese renderings.

The same Tibetan word *dgoṃs* (-pa), "meaning (of sacred texts)," is similarly used to render the word *saṃdhā* (= *saṃdhi*) at SP 125.3 (*dgoṃs* to *bśad-pa* = *saṃdhā-bhāṣitam*). Oftener, however (as far as my experience goes), the Tib. uses a fuller expression for *saṃdhā*, viz. *ldem-por dgoṃs* (-pa), which means "enigmatic or riddlesome meaning." (So e. g. in rendering SP Kern-Nanjio 29. 7, 34. 2 and 10, 60. 12, 62. 11, 125. 2.) From this come the renderings (also in Chinese, as noted by Lamotte and Ware) "mystery" and the like. But when Lamotte, as quoted by Ware, suggests that Tib. *dgoṃs-pa* is an "abbreviation" for *ldem-por dgoṃs-pa*, and even apparently concludes from this that Skt. *saṃdhi* may be similarly an "abbreviation" for *abhisamdhī*, he squarely inverts the process that has taken place, as I believe. On the contrary, *ldem-por* is an expansion, a free gloss, explaining more fully what the translators thought the expression really meant. In spite of the woodenness of Tibetan translators in general, they occasionally allow themselves a sufficient enlargement of the text to make clear its real meaning as they understand it. That is what happened when they expanded *dgoṃs-pa* by the adverbial *ldem-por*.

The expansion was not wholly without justification. In Buddhist usage *saṃdhā* and *saṃdhi*, associated with a word meaning "speech, words," mean "complete, comprehensive meaning." The opposite is the "first-expressed" message of the Buddha, which evidently comes very close to the "*prima-facie*, superficial meaning" of his gospel. It is important to note that *saṃdhā* and *saṃdhi* (in the meaning under consideration) are regularly associated, often compounded, with words meaning "speech, words." Thus *saṃdhā-bhāṣya* SP 29. 7, 34. 2 and 10, 39. 11, 60. 12, 70. 5 and 8, 273. 14, 337. 2; *saṃdhābhāṣita* SP 125. 2 and 3, 199. 2, 233. 11, 288. 2; *saṃdhāvācana* SP 59. 4 and 5; *saṃdhāya* (Prakritic oblique case,

doubtless instrumental, of *saṃdhā*) . . . *bhāṣitam* SP 63.11; *saṃdhāya vakyaḥ* SP 64.7; *saṃdhāya* . . . *bhāṣita* SP 394.1; *yaḥ saṃdhāya* . . . *saṃvārpanām kurotī* SP 34.4; *na bhāṣate bhūtapuṣṭrīhasaṃdhīḥ* SP 118.2, "he (Buddha) does not declare the real (true, *bhūta* as in Pali, very common in BSkt.) whole-essence of the meaning of his words."

One of these passages deserves attention because it furnishes, I think, the clue to the solution of the whole matter. This is SP 60.12 i.: *asmābhir . . . saṃdhābhāṣyam bhagavato 'jñanamānais teera-mānais prathamaabhāṣitaiva tathāgatasya dharmadeśanā śrutva-dyghātā* . . . "we, not knowing the complete (fundamental, essential) preaching of the Lord, in our haste heard and accepted only (note *eva*!) the first-expressed (*prima-facie*) gospel of the Tathāgata." That is, we did not wait to learn the full purport of his message, to be initiated into the whole of the doctrine, but thought that we had enough when we had heard his first words. As usual, Tib. says *ldem-por dgeṇs-te bāad-pa*, "words of enigmatic, mystic meaning"; as a free interpretation this is doubtless not far from correct, but it is not what the Sanskrit says literally. The context makes it very clear that the "first-expressed" (and superficial) doctrine is the Hīnayāna, while the "complete, comprehensive" (according to Tib. "enigmatic, mystic") doctrine is the Mahāyāna.

Even standard Skt. knows *saṃdhī* in the sense of "totality, whole essence (of something)." Note e. g. *Indische Sprüche* 7961 *nīti-saṃdhī*, rendered by Hochlingk "das Ganze der Lebensklugheit." Among the meanings quoted by Hindu lexicons for the noun *saṃdhā* are *maryādā*, *sthiti*, *avadhī*, "limit, fixation, extreme end." The verb *saṃ-dhā* (see BR.) is used in senses like "zusammen-fassen, to comprise, include (completely)." These definitions suggest the origin of the use of *saṃdhā*, *saṃdhī* in BSkt., primarily in association with terms meaning "speech, words," in the sense of "complete, comprehensive, (and so) fundamental, essential meaning," in contrast with "first-expressed (and so) *prima-facie*, superficial meaning."

A word should be said in closing about another Chinese rendering frequently used for *saṃdhā*, namely *sui-i* 隨宜 which is often rendered in some such way as "suivant la convenance" (Julien, quoted by Kern, *SBE* 21 p. 62 note). But among the renderings of *i* 宜 given by Couvreur are "arranger convenablement,

*régler, mettre en ordre.*" These could easily pass for renderings of the Skt. verb *sam-dhā* in many of its meanings, and it seems not too rash, perhaps, to guess that the Chinese character was an attempt at a literal rendering of the root-noun *samdhā*. Whether the translators understood by this Chinese rendering exactly what I assume to have been the real meaning of the Sanskrit, is another question, which I at least should not dare to answer. But at least I do not believe that they meant anything closely suggested by "*suiwant la convenance.*"

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## REVIEWS OF BOOKS

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*Arabs of Central Iraq, their History, Ethnology, and Physical Characters.* By HENRY FIELD. With an Introduction by Sir Arthur Keith. Chicago, 1935: FIELD MUSEUM OF NATURAL HISTORY. Anthropology Memoirs. Vol. IV, 474 Pages, 156 Plates, 48 Text Figures, 3 Maps.

The painstaking technique of Henry Field and the critical analysis of Sir Arthur Keith have united to produce a report that is classic in its thoroughness. The volume concerns itself almost solely with the anthropometry of 667 adult males measured by Field in 1928; 398 Arabs living near Kish; 231 Iraq soldiers measured in the army camp at Hilla on the Euphrates; 38 Ba'ij Beduin living northeast of Kish. On each individual fourteen measurements were taken from which nine indices were calculated, and twenty-three observations were made.

Sir Arthur Keith draws from his vast experience to envision the Arabs in their temporal and geographical relationships, making careful use of selected comparative data from Iraq, Africa, Iran, and India. His treatment of the material is qualitative rather than quantitative. The usual frequency curves are not employed. Instead, samples of fifty or less are plotted so that for each individual the compared measurements are visualized. For example, each ratio of head length to head breadth (the cephalic index) is expressed as a point on a chart where length is the ordinate, breadth the abscissa. In this fashion the following ratios are studied: cephalic length-breadth, cephalic minimal-maximal breadth, nasal height-width, facial height-breadth, and stature-sitting height proportions. Sir Arthur also relies upon a scrutiny of Field's excellent type photos in support of his conclusions.

The major thesis in Keith's racial analysis is based upon Arabia's geographical location as lying between negroid Africa and negroid (Dravidic) India, with Caucasoid Europe and Mongoloid Asia to the northwest and northeast respectively. Sir Arthur concludes that "the Arab shares traits with the Hamitic peoples of Africa, with the Dravidian and Indo-Aryan peoples of India, and with the peoples which extend from the gates of India to the Levant."

This racial heterogeneity is explained by the assumption that in earlier Pleistocene times a pigmented (negroid) race extended from Africa through Arabia and India to the Pacific. Hence the earlier inhabitants of Arabia were deeply pigmented and close kin to Hamite and Dravidian. In later Pleistocene times Whites ("Semites") and Yellows thrust southward to disrupt the continuity of negroid distribution, the former into Arabia and Northwest India, the latter into Indonesia. The White thrust dominated so that the Arab is basically Caucasian. Only vestiges of his earlier racial background remain in fullness of lip, relative sparseness of facial hair, "large, luscious gazelle-like eyes," and tendency to tightly curled head hair.

Sir Arthur concludes that the modern Arab has exactly the same cephalic type as the ancient inhabitants of Ur, except that the Arab has 100 cc. less brain capacity, a retrogression which, Keith avers, may explain cultural decay.

Henry Field presents a few notes on the evidence of prehistoric culture in the Kish area. He concludes that the earliest inhabitants of the North Arabian Desert were Proto-Mediterranean, a conclusion which the reviewer is prepared to support on the basis of the study of material from Alişar, Hüyük, Anatolia, and Tepe Hissar (Damghan), Iran. Field states that ca. 4300 the Sumerians, probably brachycephalic, swept in, only to be succeeded several centuries later by a Mediterranean people now possessing a Semitic culture. The alternation of long-heads and round-heads is found at Alişar and Tepe Hissar also, and seems to be general throughout Asia Minor and the Middle East.

Stephen Langdon offers brief notes on the historic succession at Kish and the social life and customs of modern Arabs—food, music, weapons, games, taxation, religion, education.

This is followed by the complete anthropometric record of each individual, some 325 pages of valuable raw data: measurements, indices, observations. The essential measurements of the three groups are given in tabular form.

The volume is concluded by an *Appendix* in which Field reports the measurement, in 1934, of 3056 individuals in Syria, Iraq, Iran, and the Caucasus region of the U. S. S. R. This is followed by 156 Plates of photos of Arabs, presenting a truly magnificent series of race-types.

The genius of Keith, the industry of Field, and the lavish completeness of the published volume render *Arabs of Central Iraq* a landmark in the study of an ethnic group.

W. M. KROGMAN.

*Gayā and Buddha-Gayā.* By BENIMADHAN BARUA. Vol. I, Book i, *Early History of the Holy Land*, pp. 280, and Vol. II, Books ii-v, *Old Shrines at Buddha-Gayā*, pp. 128, with 77 illustrations on 41 collotype plates. Calcutta: INDIAN RESEARCH INSTITUTE PUBLICATIONS, Indian History Series No. 1, and Fine Arts Series No. 4, 1934.

In these two volumes, of which the first is mainly occupied with a discussion of the *Gayā Māhātmya*, Professor Barua covers at greater length, but with less adequate illustration, the subject matter of my "Sculpture de Bodhgayā," *Ars Asiatica* XVIII, Paris, 1935; the dates of publication naturally preventing any use of one another's work by either author. What is most extraordinary, however, is the fact that Professor Barua completely ignores everything that has been published on the identification and interpretation of the Gayā reliefs by Buchhofer, Foucher, Vogel, and myself (see Bibliography in my work, pp. 63-64). It is just because of this that so many of the identifications of Jātaka scenes and other motifs now offered by Barua are so unconvincing and often completely unacceptable.

He is for example unaware that his "demi-god standing on a ram-like quadraped and holding a big bunch of water lilies" (p. 94 and fig. 20), first discussed by Marshall (*JRAS.* 1908, p. 1098) and later by Kramrisch and Cohn has been correctly identified by Buchhofer (*Jahrb. der As. Kunst*, II, 73 f., 1926) as a representation of Indra offering a bundle of *Kuśa* grass to the Bodhisattva about to take his seat beneath the Tree; another Indian representation occurs on an early Āndhra relief in the Museum of Fine Arts (*Bulletin*, April 1929, p. 21); and another, in which the Gayā figure of Indra the "grass cutter" is more closely paralleled as regards the royal costume, occurs at Amarāvati (Fergusson, *Tree and Serpent Worship*, Pl. LXVII, left centre, nimbate turbaned figure of Indra to left of *pādukā* at the base of a fiery pillar representing the Buddha). In the case of a Jātaka scene represent-

ing the reception of food and drink from a tree, described by Barua as "A Hermit on a Hill-top" with a doubtful reference to the *Vessantara Jātaka* (p. 115 and fig. 75), no mention is made of the fact that this scene, and its analogue at Bharhut, have been plausibly identified as illustrations of the Story of the Treasurer related in the *Dhammapada Atthakathā*, II. 1. 6,—an identification made independently by Chanda in *Mem. A. S. I.* 30, p. 6 and by myself in *JRAS.* 1928, p. 393.

Perhaps the most absurd identification is that of the well-known *Kaccapa Jātaka* scene (J. 315, Fausbøll II. 175), p. 112 and fig. 70, as an illustration of "Śakra's sermon on the evils of drinking" (J. No. 512). I do not thank the professor with having credited me (at second hand) with this discovery! The correct interpretation was established by Foucher in 1919. There is an illustration of another stage of the same *Jātaka* in the Mathurā Museum (J. 36, see the Catalogue, p. 149), illustrated in *Bérou*, 1909, No. 3, p. 21.

Barua makes only a passing reference to "3 pillars taken to the Kensington Museum and the 3 or 4 pillars removed to the Indian Museum, Calcutta" (p. 48), but does not illustrate or describe any of these. There is only one at South Kensington, and this bears an interesting representation of the *Chaddanta Jātaka*, identified by Foucher.

An enigmatic scene representing two pillars with railings in a park is described by Barua (p. 16 and fig. 12) as "Miraculous Growth of Ba-trees." B. describes the left hand "seed-box" (i. e. railed enclosure at the top of one pillar) as provided with "two umbrellas"; actually the lower of these "umbrellas" is a polycephalous Nāga.

The so-called representation of the four "Indian Graces" (p. 114 and fig. 74) which Barua connects with the *Suddhābhajana Jātaka* (No. 535) appears to us to be a scene from the *Vessantara Jātaka*, the Bodhisattva with Maddī Devi standing to left, bestowing the two children on the Brahman who stands on the right, and with divine spectators above. But here, as in many other places there can be found room for legitimate differences of opinion; and though we should be tempted again and again to disagree with solutions proposed by Barua, it would scarcely be profitable to take up every point here.

What we do feel bound to lay stress upon is that of all the

references cited above (and others could be added), not a single one is made use of by our author; and while there may be cases in which it is practically impossible to familiarize oneself with the whole literature of a subject, in the present case the field is limited, and we think that Barua's neglect of all but the oldest literature, taken together with the poverty and incompleteness of the illustrations, justify us in saying that these volumes are very far from important contributions to our knowledge of early Indian art.

MUSEUM OF FINE ARTS,  
BOSTON.

A. K. COOMARASWAMY.

*A Short Grammar of Old Persian, with a Reader.* Accompanied by a word-for-word translation, notes, and vocabulary. By T. HUDSON-WILLIAMS. Cardiff: THE UNIVERSITY OF WALES PRESS BOARD, 1936. Pp. 8, 52. Price, 5 Shillings.

So small a book can contain few details, but what is in it ought to be carefully arranged and meticulously correct. Particularly there can be little bibliography; but we fail to find any mention of King and Thompson's definitive work, *The Sculptures and Inscriptions of Darius the Great on the Rock of Behistun in Persia* (London, 1907), or of any of the collections of the inscriptions published in the last decade. This is amazing.

So also is the system of normalization of the Old Persian words. The author has in most respects followed Benveniste in his revision of Meillet's *Grammaire du Vieux-Persan*, including the ill-advised *ss* with a glide-ligature sign under it. He inserts in his words certain letters omitted in the original documents, though in some instances the sounds were pronounced. Thus he writes *h* before *i* in *Čahîšpîš*, before *u* in *Aharamazdâ*, *huvaspâ*, *humartîyâ*, before *m* in *ahmîy*, *ahmahîy*, *taukmâ*; he writes *i* between *h* and *y* in verbal endings (original *-si*), as in *ahiy*, *ahmahiy*, *vaivâhiy*; he writes *x* (velar spirant) before *uv* (*xuv* < *sy*) in *aniyâxuvâ*, *maškadâxuvâ*, *patîpayaxuvâ*, *xuvâmršîus* (which should be *-šîyus*); he inserts a nasal before stops where it is etymologically warranted, as in *ahantâ*, *jantâ*, *bandaka*, *Kasbujiya*. There is not a hint that these letters have no representation whatever in the original.

Further, he accepts (page 1) the inscription of Ariaramnes and

the short one of Cyrus, as belonging to these rulers (cf. Brandenstein, *WZKM* 39. 13 ff.). Evidently for local reasons, he often quotes Welsh cognates of OP words; there are 21 such listed in his index, and only 4 Sanskrit words—but how can a student utilize OP if he does not use Skt. forms for comparison? The meanings which he gives for OP words are not always correct: for example, the imperative *praś* (3) no longer meant “ask,” but “punish”; *aiwa* (3) meant “one,” as he states, but did not mean “only” in OP.

I pass now to some single items. The word-divider (3) is not a long horizontal wedge, but a long diagonal wedge or an angled sign; he omits mention of the ideograms. For the IE palatal stops he gives (4) the OP values *θ* and *δ*, but omits the fairly common *s* and *z* which seem to be borrowed from another dialect. The statement of the Aryan Law of Palatalization (4) is inexact, since palatalization was caused not merely by a following IE *ǵ* or *ǵ̃*, but also by *i* and by Aryan *i* from IE *e*. Latin *vōbis* (5) should be *vōbīs*, which reduces its value for comparison, and the citation of *classibus* and *lq̄* lacks relevance unless further details are given. It is a bad formulation to say (8) that common nouns with stems in *a* form the locative by adding *ā* to the original *-aiy* and dropping the *i*. *Dakṣya* (9) is nom. as well as acc. The form *akṛant* ought not to be cited as an old *s*-acrist (18, 36), since it is a new hybrid of imperfect and *s*-acrist. The word *avadaś* (34) should be normalized *avadaśa*. The form *śakata* is nom. plural of the participle, as is shown by the singular *śakataṃ* when the subject is singular; it cannot be taken as an instrumental (34), despite some scholars. The word *reuta* in *Pirana nāma reuta* is not an ablative (38), but an anacoluthic nominative phrase.

In many of his errors, the author followed his sources; even so, he should have avoided adding others of his own. If the book were free of errors it would be useful for the author's stated purpose, namely, “to provide those who study the history of ancient and modern languages with some account of an Indo-European language at approximately the same stage as Greek and Latin,” especially as he adds that it should “be used in conjunction with some short manual of Indo-European grammar.”

ROLAND G. KENT.

University of Pennsylvania.

*Modern Newspaper Chinese.* By J. J. BRANDT. Peiping: VETCH, 1935. 321 pages.

Brandt is already well-known for his *Introduction to Literary Chinese* (first edition, 1927; second edition, 1935), and *Wenli Particles* (1929). He is the first English author to give adequate attention to the use of the particles in Chinese, and on this account alone his work is of great value to students of Chinese in their study of sentence construction. His latest work contains selections from the better-written Peiping, Tientsin, and Shanghai newspapers of 1934, arranged in sixty lessons of progressive difficulty. Each selection is accompanied by a vocabulary, notes, and a translation. As one would expect, special attention is given in the notes to the Wenli particles which still play an important part in modern written Chinese. There is an index to the notes, and a vocabulary index, but the latter is not as full as it should be. The selections have been carefully made, and the translations are accurate. On page 33, *hsiu yang* 休養 is translated "to take a cure." The phrase is actually used in the sense of "to take a vacation."

No matter what field of Chinese studies a student intends to enter, he should master the modern written style. Even though he intends to handle ancient texts, he cannot be competent unless he is able to consult the work of modern Chinese scholars both in books and periodicals. The present volume could have been made more valuable to scholars if a different type of article had been selected, and it seems designed chiefly for the government services. Nevertheless the book would be useful to almost any scholar in this field, particularly for the large number of modern phrases and expressions involving more than one character.

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*Vocabularies to the Elementary Chinese Texts used at Harvard University.* By JAMES R. WARE. Cambridge, Mass.: HARVARD-YENCHING INSTITUTE, 1936. 132 pages.

It will take a long time before any American university will be able to develop a department of Chinese studies which will compare with the important centers of European learning in this field. But a beginning has been made, and this volume of Dr. Ware indicates that the Harvard-Yenching Institute is building on a firm founda-

tion and making material progress. The book is a vocabulary to be used by students in the elementary class in Chinese, and contains 1,302 words. This is a heavy assignment for an elementary class whose members probably have other work in the university. It is, the reviewer believes, rather more than the number of words given to elementary classes in language schools in China, where the pupils devote at least five hours a day to the subject. The comparison is not exact, however, since in China the students are primarily learning to speak and use Chinese, while at Harvard they are learning only to read. It remains true that the discipline is severe, as it should be, though it is to be hoped that the severity will not be great enough to discourage students. The Chinese language is not easy to learn, and the first years of its study are very trying.

After learning forty-one common characters, the students pass to Chinese texts; first, the *Fu-hsing Textbook of History* by Hsi Ying-ch'uan, and then the *History of Vernacular Literature*, by Hu Shih. Some idea of the difficulties involved for elementary students is given by the fact that the *Vocabulary* indicates that seventeen pages of the first, and thirty-four pages of the second volume are covered.

The *Vocabulary* has been carefully prepared. Characters are given both in the printed form and in the clerk hand. For the Introduction and the *Textbook of History*, large type is used. The part dealing with Hu Shih is provided with notes. Each character is accompanied by its Wade-Giles romanization, the tone, the classification in the *K'ang-hsi Dictionary* (which seems a work of supererogation for elementary students), and the definition. The definitions are limited to the meanings which the student will need, and are not too involved for his memory. There is no treatment of the grammar, and no index. On page 121, Ta-ch'i is misprinted Ta-ch'i'i.

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*Grundriss der Ju-Lehre.* By KITAMURA SAWAKICHI. Tokyo: MARUZEN, 1935. six + 373 pages.

The author of this work is a professor in the state university of Hiroshima. The text was originally written in Chinese, and was published in Shanghai by the Commercial Press in 1928. In 1930, the Japanese text was published in Tokyo by Sekishein. In this



German translation the Shanghai text has been used, and the romanization of Chinese proper names follows Wade-Giles. Where the characters refer to a Japanese name, the Japanese pronunciation is followed in romanization. There is a "Register" or index, a table describing the development of the "Confucian Way," and a photograph of the author, who has a fine, scholarly face.

The book represents a type of which the reviewer stands in awe, but which he dislikes to read. Massive Chinese learning is added to meticulous German thoroughness. Quotation follows quotation, page after page, until the wood cannot be seen for the trees. The whole field of Confucian literature appears to have been combed for references. The book contains a tremendous amount of learning, and there is no padding. Perhaps some of the difficulty for the western reader is due to the fact that the book is written from an Eastern, and not from a Western point of view, even though the terms are translated into technical German. The author is interested in the Confucian teaching and the Confucian Way. Their origins and development, and even the origins of important words, are examined with great care. The written terms are broken up, and the component parts analysed from the definitions of the *Shuo wen* down. Much is said about the threefold division of Heaven, Earth, and Man, and the Way of each, as well as the combination of the three in the "Way of the Ruler" (*wang tao*). Eventually a whole system is developed, including morals, ritual, regulations of various sorts, self-cultivation, and other things.

The approach and interest are not ours, and it would be difficult for a westerner to be fair either in appreciation or criticism. It may be pointed out that the references might often be more exact and the notes fuller. For instance, a quotation listed as from the *Fan yen* is too vague to be checked. Again, on page 36, reference is made to Wen Weng as emphasizing the *Spring and Autumn Annals* in the district of Shu; and a note says only that he lived during the Han period. The note could easily have added the reign periods in which Wen Weng lived, the fact that his biography is given in the *Ch'ien Han shu*, and a summary of his work in developing education in the province of Szechuan. Similar criticisms of a minor sort might be made. Whether it is possible to find exact equivalents in technical German for Chinese terms is a question. It is certainly unfortunate that both *yi* 藝 and *shu* 術 are trans-

lated "Kunst." As a whole, while the book is difficult to read, it contains a great amount of valuable material, and appears to be a careful study of a sort that could hardly be made by any western scholar.

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*A Study of the Tattooing Custom among the Li of Hainan Island.*

By CHUNG-SHER H. LIU. Shanghai: THE SCIENCE SOCIETY OF CHINA, 1935(?). 36 pages, 16 plates.

The author of this monograph is a graduate of Oxford University, the editor of the Science Society of China, and a former professor of anthropology at the National University of Tsingtao. He received his education in ethnology largely in England.

There is no area of equal significance with China and South-eastern Asia that has received so little attention from ethnologists, and therefore any efforts of this sort are very welcome. Tattooing occurs in many places. It was known to exist in ancient China, particularly among the "Southern Barbarians," and still survives among the indigenous tribes of Hainan, off the southeastern coast of China. This monograph is the result of an expedition to the island in 1934. The author shows a knowledge of the literature on tattooing, and of what work has been done in the area, as well as an acquaintance with Chinese literature. But the paper is primarily concerned with field work, and the author does not commit himself to any particular theory. Only the women engage in the practice, which is followed among more than ten tribes of the island. Four types of design were found. Faces, hands, and legs may be tattooed, and it is usually done before marriage. The text of the article is in Chinese, but there is an English abstract, and the titles of books and illustrations are given in English. The monograph is not only worth-while in itself, but is significant of the growing interest in ethnology among scholarly-trained Chinese.

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*Cults and Legends of Ancient Iran and China.* By SIR J. C. COYAJEE. Bombay: KARANI, 1936. 308 + xii pages.

This interesting volume is concerned with parallels between the "Cults and Legends" of Persia and China in general, and in particular, between Firdausi and a Chinese work called the *Feng*

*shen yen yi* 封神演義. On the Persian side the author appears to be fully competent. He goes to the original sources, and his work is commended by Professor A. V. Williams Jackson. He is unable to consult the Chinese text of the *Feng shen yen yi*, but is familiar with Grube's translation, and with the work of such sinologists as Granet, De Groot, De Harlez, Doré, Laufer, Werner, and Legge. Under the circumstances, he is as well-prepared as any one man could be to undertake such a task, though it is probable that a collaboration between scholars in such widely different fields would be more advisable.

Laufer has already shown in *The Diamond* how tales travelled across the width of Asia, so that the thesis of such diffusion does not have to be established. In this study, the author has shown such remarkable similarity between the *Shahnamah* and the *Feng shen yen yi* that it is difficult to doubt that there was diffusion somewhere. This is a valuable result, and raises many interesting problems, but unfortunately the author does not solve them. He is not much interested in dates, and makes little attempt to show the progress of the diffusion across the continent, even though this might prove to be an impossible task. But his chief errors are due to his inability to handle his Chinese source, and his necessary dependence upon secondary authorities who have sometimes misled him. Neither Doré nor Werner is critical in dealing with myths, although in the present state of sinology, the author could have found no better western authorities.

The author says (p. 99), "The latter poem (the *Feng shen yen yi*) is the most popular and best known collection of poetic and religious legends in China. . . . The Chinese epic deals with . . ." The theory is advanced that the Saka people supplied most of the legends to both Persia and China. It might be remarked that the Chinese book is not an epic, but a fanciful romance or novel. Most careful scholarship would be needed to determine whether there is a core of real folklore in the book.

The author and the date of the *Feng shen yen yi* are both unknown. The title was first mentioned in the preface to the 平妖傳 by 張無咎, which was composed in A. D. 1620, according to the 中國小說史略 of 魯迅, pp. 187-91. Therefore the compilation of the *Feng shen yen yi* cannot be later than 1620. Compared with *Firdausi*, not to mention the *Sakas*, it is obvious that it is

many centuries later. As to the date of the legends contained in this very late work, nothing can be said, but it is obvious that if there has been diffusion, it has been from Persia or Central Asia to China.

It is very interesting if a relatively modern Chinese novel can be shown to have borrowed its incidents from Persian sources, although this is not what the author attempts to prove. The fact that the scene of the *Feng shen yen yi* is placed at the close of the Yin period means nothing. It would also be interesting if diffusion could be shown in the accounts of such relatively modern Chinese deities as Wen Chang, or in the story of the Golden Caterpillar. Unfortunately the drawing of parallels is not enough, although the thesis is quite likely.

The format could have been made more attractive, and there are many mistakes in the romanization of Chinese names.

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*Historical and Commercial Atlas of China.* By ALBERT HERRMANN.  
Cambridge, Mass.: HARVARD UNIVERSITY PRESS, 1935. 112  
pages, 60 maps.

It is difficult not to be enthusiastic over this much-needed atlas. Dr. Herrmann, the Professor of Historical Geography in the University of Berlin, has placed all teachers and students of Chinese history in his debt by a careful and admirably executed piece of work which should have been done long before. The maps have been designed by him, drawn and printed by Georg Westermann, and some give the authorities (Haloun, Maspero, etc.) according to whose account the maps have been constructed. It might be noticed that where such authorities are given, they are usually western scholars, although in most instances the final authorities are necessarily Chinese. The largest maps are approximately thirteen by eleven inches, many are smaller, and there are numerous insets. The last thirteen maps deal with modern political and economic conditions, but the remainder are historical. There is no text in the volume, but there are splendid bibliographies, an index, and a list of Chinese characters, which are not given with the maps. The bibliographies give Chinese as well as western authorities, such as Mu Ch'ien's *Geography of China at the Beginning of the Chou*

*Dynasty*, but most of the works mentioned are western. Books as recent as 1934 are included.

It is obvious that a work of this kind is designed to give general and comprehensive points of view, rather than minute and detailed information. For instance, one would require larger and more detailed maps in order to work out the military campaigns of Lin Pang or Chu-k'o Liang. And a western scholar unacquainted with Chinese would find difficulty identifying the names on the T'ang map with those on the Ming or Ch'ing, especially as there is no way of discovering how the place names have altered during the centuries. These are inevitable limitations, and merely mean that the scholar must supplement this atlas with other works. For the teacher of Chinese history, the volume will be invaluable, especially if he is giving a course on China without being familiar with the language. A good many historical maps of this sort have been printed in China, although none that the reviewer has seen are so good as these, but practically nothing of the kind has existed in English. The book is especially to be praised for showing the relations of China with the rest of Asia at various periods, with the trade routes and itineraries of travellers.

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*Foundations of Chinese Musical Art.* By JOHN HAZEDEL LEVIS.  
Peiping: Vercin, 1936. xiii + 233 pages; 1 plate.

During the years 1931-34, American audiences had the privilege of listening to Mr. Levis' lectures and recitals. A part of the material he gave is now published in book form. The book deals with Mr. Levis' theories concerning Chinese music, and in particular with the study and analysis of nine music-poems of the T'ang and Sung periods. The author is singularly well-fitted for his task. Born and raised in China, he has actually resided in the country for twenty-five years. He is, moreover, thoroughly trained in the theory and practice of western music. The book is a serious and scholarly treatment of a difficult subject, practically untouched by western students. It does not pretend to be either complete or final. It is primarily analytical and appreciative rather than historical, and does not attempt a historical account of the development of Chinese music. Mr. Levis is to be praised for the rigid limitations he has set himself, and for his faithfulness to them.

He does not attempt to go farther than the 6th century of our era, and practically confines himself to one type of composition. He appears to demonstrate conclusively that Chinese music is intimately connected with the tonal structure of the Chinese language, and this is probably his chief theoretic contribution. His characterization of Chinese music as tone-painting is admirable. He claims that Chinese musical documents contain "a whole world of music which illustrates the only art of melody in any known musical system." There has been some criticism of his theories by Chinese.

The heart of the book is concerned with the analysis of the nine music-poems, which is technical and appears to be very thorough. The Chinese of the poems is given, the Chinese musical notation in Chinese characters, arrangements of the music in European notation, an English translation, and the modern romanization of the Chinese. This last is of little value, because of the great changes in the pronunciation of Chinese since the time of composition, and it would have been better if the author had used Karlgren in working out the ancient pronunciation of the words.

The reviewer is glad to have a suspicion of his own confirmed by Mr. Levis—that the intervals and rhythm are left largely to the individual performer. He is also glad that Mr. Levis' contacts with Chinese music have been more profitable than his own. It seemed that whenever the reviewer heard a good song, and learned it, he was always told that the words were such that a clergyman should not even be heard humming the tune. It would be a help to scholars if in his future researches Mr. Levis should go into the purely historical aspect of Chinese music a little more thoroughly, and perhaps examine such music as that played at the sacrifices to Confucius, when a fairly large number of instruments was used. It might be pointed out that in the table of sounds made in the Liang period (A. D. 502-56), and given on p. 231, the typically Chinese division of five is used, and the classes are named after the five elements; wood, fire, water, metal, and earth. In this table, the word 徵, should be romanized *chi* when it indicates a musical tone, and the romanization of 角 should be *chie*, instead of the colloquial *chieh*.

The book contains seventeen figures, or diagrams, an index, and excellent European and Chinese bibliographies.

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*Le Droit Chinois.* By JEAN ESCARRA. Peking: VETCH, 1936.  
xii + 559 pages.

In a brief review of a monumental work of this kind it is only possible to indicate the scope of the work, and its general value. M. Escarra is known as the leading western authority on Chinese law, on which he had published six books and twelve articles before the present work. He is a Professor in the Faculty of Law of the University of Paris, and adviser to the Chinese Government. No living man has made such a thorough study of the questions of legislation, codification, jurisprudence, judicial organization, and allied subjects as they occur in China. While the author himself calls his work a sketch, it is certainly the most complete treatment of the subject in any western language. It is not a translation of Chinese sources, but an original treatment based on those sources. It is divided into four parts. The first treats of the Chinese conception of law; the second, of legislative institutions; the third, with the details of judicial organization; the fourth, with instruction in the study of law and the provision for scientific research. There is also a general conclusion which sets forth the author's views on the material he has given. There is an index, and a classified bibliography which occupies fifty-one pages. Chinese characters are given, and full footnotes.

The sinologist will not be so much interested in the present conditions and tendencies in Chinese law, as in the historical accounts. The second, third, and fourth parts each begin with an historical chapter, and these justify the author's use of the word sketch, since in such limited space it is impossible to give more than summaries. In these summaries, the author generally follows Shen Chia-pen. In the first part, dealing with fundamental ideas, the author appears to depend too much upon Granet. The chapter on the School of Law devotes seventeen pages to an analysis of seven chapters from Han Fei, and there is a good discussion of Buddhist influence.

No brief account can do justice to a really great work, and it is not too much to say that for a long time this book will remain the standard in western languages upon its subject, even though it may raise more questions than it solves.

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*Annual Customs and Festivals in Peking.* By TUN LI-CH'EN, translated and annotated by DR. BODDE. Peiping: VICTOR, 1936. xiii + 147 pages, 6 plates, 28 text-drawings, 1 map.

Laufer used to say that investigations of religion should begin with the calendar. This is what Mr. Bodde has done in translating the *Yen-ching Sui-shih-chi* of Tun Li-ch'én, and in doing so has introduced us to a pleasant and valuable little book. The author of the original was a Manchu, who composed the work in 1900, and died in 1911. The book runs through the year, month by month, recording the chief Peking observances. The accounts are very brief, and the number of observances varies from twenty-three in the twelfth month to five in the second. There are six appendices, a bibliography, and an index.

The text is tantalizing, for there is hardly an item about which one would not like to have more information. Many of the observances are found all over China, but some are peculiar to the old Peking. In nearly every instance, a monograph might be written in explanation of the observance, and it may be said of practically all of Mr. Bodde's notes, that while they are accurate as far as they go, and adequate for the general reader, they are not sufficiently thorough and detailed to satisfy the scholar. There are eleven lines of notes on the Yü Lan Hui, and twenty lines of text; fifteen lines of notes on the Dragon Boat Festival, and eighteen lines of text. The notes at least should have been much fuller. There are four lines on realgar wine, and no note mentions that realgar is a compound of arsenic, and was used in elixirs of immortality. In general the material of the text is good, and the notes carefully done, but it is a great pity there is not more of both. The format is attractive, and the illustrations excellent.

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*The Spirit of Zen.* By ALAN W. WATTS. New York: DUTTON, 1936. 136 pages.

The *Wisdom of the East Series*, edited by L. Cranmer-Byng and Dr. S. A. Kapadia, is a set of small books intended primarily for the general cultured reader, rather than for scholars. It contains some valuable studies, such as the translations of Taoist works



by Lionel Giles. The present volume, however, is one of the less valuable ones, at least from the standpoint of the scholar. It is not a critical study, and seems to be merely a popular presentation of the work of Suzuki, whose influence is properly acknowledged. As a popular presentation of Zen positions it will doubtless be useful, although it seems questionable whether there is any value in talking about experiences which transcend the grasp of the intellect, attractive though such things may be to some people.

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*The West Chamber.* By HENRY H. HART. California: STANFORD UNIVERSITY Press, 1936. xxxix + 192 pages.

This Chinese drama was written by Wang Shih-fu, who flourished in the middle of the 13th Century, that is, during the Mongol period. It is based on a story by Yüan Chen, of the T'ang period. The play has already been translated into two European languages; into French by Stanislas Julien (1872-80), and into German by Vincenz Hundhausen (1936), but this is the first English translation. It follows the edition published in Shanghai in 1931 by the Ch'ün Hsüeh She, which contains fifteen acts. There is a foreword by E. T. Williams, a preface and introduction by the translator, good notes, and a bibliography of four pages which includes fifteen Chinese works. Chinese characters are given in the notes and the bibliography. The latter includes collections of Yüan dramas and critical studies by modern scholars.

In this volume Dr. Hart has performed a real service. The translation reads well, and the scholarship appears to be sound and thorough. The introduction is based on the work of Wang Kuo-wei, who is generally recognized as the leading Chinese authority upon the drama. It is easily the best account of the Chinese drama to be found in English. A few suggestions may be made on points which Dr. Hart does not bring out.

The word *hsiang*, translated "chamber," is used for certain rooms in a Chinese dwelling. Chinese houses theoretically face the south, and contain courtyards surrounded by rooms. The *hsiang* are rooms on the eastern and western sides of the court, so that "The West Chamber" refers to an exact location. It is hardly accurate to refer to the *new* as practicing "black arts" (p. xvii), since the

magic of the wu was beneficial, or to refer to the "dour and savage leaders of the wild hordes of Genghis Khan and his descendants" (p. xx), since audiences of the time of Kubilai may have been highly cultured. The author does not point out that while great license in speech is permitted in Chinese plays, actions and costumes are strictly regulated and are highly proper. Nor does he mention that the Chinese drama proper, which is somewhat like our opera, is paralleled today by realistic plays which are extremely well-done, even by amateurs, and in which western scenery is sometimes used. He does not point out that there are different types of music, requiring different instruments, used with different styles of drama. These points are minor, however, and Dr. Hart has done an excellent piece of work.

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*Selling Wilted Peonies.* By GENEVIEVE WINSTATT. New York: COLUMBIA UNIVERSITY PRESS, 1936. xi + 120 pages.

The reviewer confesses considerable sympathy for the attempt made in this book. Scholarly accounts are usually recitals of facts which leave untouched the living reality. Genevieve Winstatt has endeavored to re-create, not so much the life, as the personality, of the T'ang poetess, Yü Hsuan-chi, and to accomplish this she has elaborated the little that is known of her subject's life, added considerable background of her own, and interpolated into the account translations of Yü Hsuan-chi's poetry. The scholarship behind this study is carefully concealed, and is subordinated to the artistic side. Whether this particular attempt can be called successful is another matter. The author's style is rhapsodic, and strains after effect. It is a mistake to use rhyme in translating Chinese poetry, even though it occurs in the original. The story as a whole is well-handled and the sentiment of the poetry is moving, but the reviewer doubts that Miss Winstatt can describe the state of mind of a Chinese girl of the 9th Century.

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*The Sutra of the Lord of Healing.* By WALTER LIEBENTHAL.  
Peiping: THE SOCIETY OF CHINESE BUDDHISTS, 1936. ix  
+ 30 pages; 1 illustration.

The Society of Chinese Buddhists projects a series of translations of Buddhist documents, of which this translation by Dr. Liebenthal, of the National University of Peking, is No. 1. New Buddhist translations are always welcome. This English version is based on the Chinese translation of Hsüan-tsang. One section has already been translated by T. Richard (*The New Testament of Higher Buddhism*, Edinburgh, 1910). The sutra belongs to the Mahāyāna, and is concerned chiefly with Mañjuśrī, whose twelve vows are given. Proper names are given in the Sanskrit forms, not the Chinese. Unfortunately no critical apparatus is given.

J. K. SHRYOCK.

Philadelphia.



## NOTES OF THE SOCIETY

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Baron Dr. Alexander von Staël-Holstein, corporate member of the Society, died March 16, 1937.

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## NOTES OF OTHER SOCIETIES

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The Fifth International Congress of Papyrology will be held at Oxford, 30 August-3 September, 1937. Persons intending to take part in the Congress are particularly asked to note that the Final Circular will only be sent to those who have communicated with the secretaries of the Congress. Notice of the intention to read a paper should be sent not later than June 30 to Mr. C. H. Roberts, St. John's College, Oxford, to whom all enquiries should be addressed and changes of address notified.

The Oriental Commission of the Warsaw Society of Sciences announces that it is about to start an annual Bulletin devoted to the critical review of all new publications in the field of Oriental researches, viz., on the Old Testament, Christian Orient, Ancient Near East, Islamic and Altaic Studies, India, Iran, Far East, etc. The reviews will be written by Polish and foreign specialists in English, French, and German. The first issue is expected to come out by the middle of June, 1937.

The Department of Egyptian and Assyrian Antiquities, of the British Museum, announces that the Students' Room of this department on the upper floor will be open on Monday, May 10th, 1937 and thereafter regularly, with the exception of the usual holidays, for the use of students of Egyptian papyri and Assyrian and Babylonian tablets. Other classes of written documents and the archaeological material will be made available as the re-arrangement of the collections is completed.

Dr. Fang-Kuei Li has been appointed Visiting Professor of Chinese Linguistics at Yale University for two years, beginning next fall. Dr. Li is a brilliant and thoroughly trained linguistic scholar. He studied Indo-European, American Indian and General Linguistics at the University of Chicago (Ph.D., 1928), under Professors Buck, Sapir, and Bloomfield. Since his return to China he has done distinguished work, notably in Chinese and Tai dialects, as a member of the research staff of Academia Sinica (Nanking), which has now granted him leave to found the first school of linguistic science in the Far Eastern field in America. He is expected to arrive in this country in September; in the meantime, Professor Franklin Edgerton, Chairman of the Department of Oriental Studies in Yale, would be glad to hear from any scholars or advanced students who might wish to take work with Dr. Li.

PROCEEDINGS  
OF THE  
**American Oriental Society**

AT THE JOINT MEETING WITH THE MIDDLE WEST BRANCH  
CLEVELAND, 1937

The One Hundred and Forty-Ninth Meeting of the Society, a joint meeting with the Middle West Branch, was held at Cleveland, Ohio upon the invitation of Western Reserve University, the Cleveland Art Museum, and the Cleveland Public Library. The Society visited or held one session at each of the inviting institutions. Four additional sessions were conducted at the Hotel Statler. The following members participated in the sessions:

Abbott, Miss	Evans	MacLean
Albright	Feigin	McCallough
Avery	Folkman	McGovern
Barret	Fullerton	McNair, Mrs.
Bender	Gaskill, Miss	Martin
Bingham	Ginsberg	Matthews, I. G.
Blair, Miss	Goetze	May
Blank	Goossens	Meek
Bobrinsky	Gordon	Menzies
Braden	Grant, E.	Nicholson
Brown	Graves	Mihelle
Buckler	Gruenthaner	Morgenstern
Burrows	Hahn, Miss	Nims
Butin	Hall, R. B.	Nyckl
Buttenwieser	Hamilton	Ogden, C. J.
Cameron, C. G.	Hardy, R. S.	Orvis, Miss
Cameron, Miss	Harris, Z. S.	Perkins, Miss
Coomaraswamy	Hauptert	Plumer
Creel	Hollis	Price
DeWitt, Mrs.	Irwin	Purves
Dubs	Keogh	Pyatt
Dumont	Kruehling, C. H.	Reich
Duyvendak	Kraft	Rigg
Eames	Latourette	Roach
Edgerton, F.	Lewy	Sage
Engberg	Ludwig	Sakanishi, Miss
Enslin	Lybier	Sauer

Schurman, Miss	Stefanski, Miss	Well, Miss
Sellers	Stehle, Miss	White
Seraphin	Stinespring	Wilbur
Simsar	Sturtevant	Williams, W. G.
Smith, Miss L. P.	Totten, Miss	Wilson
Sommer	Uhl	Warrell
Sprengling	Waterman	Wright
Staples		Total 103

### FIRST SESSION

The first session of the meeting was called to order by President Edgar H. Sturtevant at 10:15 A. M., March 31, in the Salle Moderne of the Hotel Statler.

Professor Francis W. Buckler reported on behalf of the Committee on Arrangements and the Cleveland Oriental Club, welcoming the Society to Cleveland. He announced that the Rowfant Club, and the Faculty and University Clubs of Western Reserve University had extended the use of their facilities to members of the Society during the sessions, and called attention to the exhibitions of Chinese, Egyptian, Indian and Indonesian, Japanese, and Persian Art at the Cleveland Art Museum, and the special exhibits of Tibetan books, Chinese Mohammedan literature, and Arabic and Persian manuscripts in the John G. White Collection of the Cleveland Public Library.

Greetings were extended to the Society at the occasion of its Cleveland meeting by Miss L. A. Eastman on behalf of the Cleveland Public Library, and by President W. G. Leutner on behalf of Western Reserve University. President Sturtevant expressed the thanks of the Society for the greetings and the arrangements made by the inviting institutions for the Society's reception.

### REPORT OF THE SECRETARY

The Secretary-Treasurer presented the following report on his work as Secretary of the Society:

Since taking office on May 1, 1886, I have tried in some measure to accomplish the purposes which the Society had in mind in appointing one man to be its Secretary, its Treasurer, the business manager of its publications, and its general factotum. This has proved to be a difficult task, for the life of the Society is relatively intricate for its size, and the administration of its affairs requires continuous application to a great deal of detail.

As Secretary my primary concern has been with the question of membership. A new way of keeping our membership rolls straight, of recording dues payments and changes of address was inaugurated. It was found that of the 685 members on the rolls on May 1, 1936, a relatively large number was in arrears of dues. Attempts were made to revive the interest of those who had fallen behind, but ultimately it was found necessary to drop most of them. During the past year seven more have resigned and death has deprived us of our association with twelve others. The deceased members are as follows:

**Honorary Members:**

Professor C. SNOOK HUMBONJE, University of Leyden, who died June 26, 1936.\*

Professor ANTOINE MEILLET, Collège de France, who died Sept. 22, 1936.\*

Professor MORIZ WINTERITZ, University of Prague, who died Jan. 9, 1937.\*

**Honorary Associate:**

Field Marshall VISCOUNT ALLENBY, general in the World War, conqueror of Palestine, peer of the English realm, holder of many honorary degrees. He was elected Honorary Associate of the Society in 1922, and died at the age of 75 on May 14, 1936.

**Life Members:**

Former President, Professor RICHARD J. GOTTHEIL, who died May 22, 1936.\*

Mr. EFTINGHAM B. MORRIS, former President of the Girard Trust Company of Philadelphia, who had been a member since 1920, and died at Ardmore, Pa., on Jan. 22, 1937.

**Corporate Members:**

Professor G. THEODORE BENZE, former President of Thiel College, for many years Professor of Old Testament at the Lutheran Theological Seminary at Philadelphia, and member of the Philadelphia Oriental Club, who died on July 4, 1936, having been a member of the Society since 1916.

Sir HENRY WELLCOME, scientist, noted benefactor of British medicine, manufacturer, patron of archaeological exploration, with interests in the Near and Far East, who died July 26, 1936, having been a member of the Society since 1928.

Professor LESLIE E. FULLER, Professor of Old Testament at Garrett Biblical Institute, a member of the Society since 1918 and President of the Society's Middle West Branch in 1928, who died on August 3, 1936.\*

Rev. BARNET A. ELIAS, Rabbi of the Beth-Miriam Congregation in Long Branch, N. J., author of a learned work on the Jewish colony at

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\* See special minutes on the death of these members elsewhere in these Proceedings.

Charleston, S. C., contributor to the Dictionary of American Biography, a director of the Jewish Academy of Arts and Sciences, devoted to the assistance of the deaf and the dumb of his own race. He became a member of the Society in 1923, and died on Oct. 18, 1930, at the age of 68.

Professor RUDOLF M. RIEMSTADT, Professor of Fine Arts at New York University, an authority on Islamic and Near Eastern Art, who joined the Society in 1930 and died on the last day of the same year at the age of 56.

We have also learned of the death of Dr. NAJEEB M. SALEEBY, who had been a member of the Society since 1922 and died at Baguio, Philippine Islands, on December 18, 1933.

In the effort to offset, as far as possible, the grievous losses suffered by the Society during the year, the Committee on Membership provided for by the new constitution and elected by the Executive Committee, began a systematic attempt to win new members. This effort, conducted with the help of the present membership, and handled through the office of the Secretary, was highly successful, resulting in the election of 114, of whom 92 have qualified to date by the payment of their initial assessment. The Society's thanks are due to the Committee on Membership and to its chairman, Professor K. S. Latourette, for their excellent work. It should be noted by the Society that of its new members no less than 68 have a special interest in the Far East. Counting only those who have qualified to date, the present membership of the Society is thus 653, of whom 23 are one year in arrears of dues. The membership list includes no one as much as two years in arrears. Fifty-five others have been elected by the Executive Committee by mail vote and at its session here, and have yet to qualify.

A great deal of my work as factotum of the Society has been of a financial character. With the advice of a professional accountant, a bookkeeping system, as simple as possible under the circumstances provided by the intricacy of the Society's business transactions, was set up. This is now functioning regularly and well. An actual account of our finances will be given later. Here I have merely to say that the cash turn-over in the year was about \$25,000, that on December 31, 1930, our total assets had risen about \$600 from the previous year, due particularly to an addition of \$607.00 to the bequest of Justin E. Abbott, while liabilities dropped about the same amount, that during the year about one-fourth of our capital was re-invested with the help of the Investment Committee, and that we stayed within the budget adopted by the Executive Committee at its session in New Haven. One of our funds, a trust fund of \$10,000 from the Estate of James B. Nies, managed by the City Bank Farmers Trust Company of Brooklyn, and over which we have no control, remains unproductive. We are, however, watching the management of the fund and ultimately hope to obtain income from it for the Society.

To manage the sales of the Society's publication other than the JOURNAL a stock room and distributing center was established in quarters adjacent to the Society's Library in the Sterling Memorial Library of Yale Uni-



versity through the kindness of Mr. Keogh, our Librarian and the Librarian of Yale University. An advertising folder was printed and sent not only to all members of the Society, but to all larger University, College and Public Libraries in this country and to a long list of foreign libraries. Comparative figures for the business done in the sale of publications in previous years are lacking, but the sales of monographs and offprints brought in slightly over \$300 in the calendar year 1936, and are running ahead of this figure in 1937. Without more advertising and strict book-keeping we cannot hope to make a success of the publication business. Members can help by bringing the publications of the Society to the attention of the librarians of the institutions with which they are connected.

In the business management of the *JOURNAL*, with which I am also charged, one important and certain other slight changes have been made. We have transferred the second-class mailing privilege from New Haven to Baltimore, expecting thereby to save the cost of sending the whole of any one issue to New Haven before mailing, and to expedite the receipt of the *JOURNAL* by members. The wrappers for the mailing of each issue are now checked against the membership file regularly, to assure correct addressing. We cannot, however, guarantee the receipt of publications unless you inform us promptly of changes of address. According to the constitution, I am not permitted to send the *JOURNAL* to any one who is one year in arrears of dues.

The Society should note that the sum allotted to me for the conduct of the Society's business has been used in the past year to defray the cost of the Membership Campaign, the handling, advertising, and sales of our monographs, controlling membership and dues payments, investments and general finance, conducting the business of the Executive Committee, arranging for the annual meeting, its program, abstracts, and minutes, preparing reports on the Society's affairs, keeping the addressograph files up to date, and many other matters.

Three things the Society needs to do in the immediate future. First, it needs to support and serve the special interests of its members. These interests are for the most part highly specialized and cover a wide range of fields of study. Unless due recognition is given to all of them, we cannot hold our membership, much less increase it. Second, it needs to make a systematic effort to increase its endowment, for the income from dues is scarcely sufficient to publish the *JOURNAL*, and the index figure of returns on investments is now much lower than formerly. Third, it needs to be reminded of the fact that in 1942 it will celebrate the hundredth anniversary of its existence. Such anniversaries need to be foreseen well in advance if they are to be prepared for effectively. One of the things the Society might well undertake at once in preparation for this anniversary is to collect the material bearing upon its own history.

Respectfully submitted,

CARL H. KRAELING,  
*Secretary-Treasurer*

It was voted to accept the report of the Secretary.

Professor W. R. McGovern presented the following minute on the death of Professor Fuller, on behalf of a committee consisting of Professor C. S. Braden and himself:

Professor LESLIE ELMER FULLER of Garrett Biblical Institute and Northwestern University met his tragic death by drowning on August 8, 1930, in the Rat River not far from the Arctic Ocean, whither he had led an expedition consisting of his two sons and two other young men during last summer's vacation. He was born in California December 28, 1882. He held degrees from Pomona College, Drew Theological Seminary, Columbia University, and the University of Bern. After a brief work in the pastorate he was called to Garrett, where he became Professor of Old Testament Literature. For the past fifteen years he also gave courses in Northwestern and for ten years was head of the Department of History and Literature in that institution. He was a contributor to the *Abingdon Commentary* and various periodicals. On the organization of the Middle West Branch he joined the American Oriental Society, in 1916, and since then was regularly active in the meetings of the Branch, which he served on several committees and of which he was president in 1928-29. His associates, particularly in the Middle West Branch, will greatly miss his genial presence and the deep interest which he manifested in the Society's affairs.

CHARLES S. BRADEN,  
WM. R. MCGOVERN.

The minute was accepted with a rising vote.

The Secretary presented the following minute on the death of Professor A. Meillet on behalf of a committee consisting of Professors R. G. Kent and W. N. Brown:

ANTOINE MEILLET, Member of the Institut de France, and Professor at the Collège de France and the École des Hautes-Études, was one of the most distinguished scholars in Indo-European and general linguistics; he was the author of important works on Indic, Iranian, Armenian, Greek, Latin, Germanic, and Slavic languages, as well as others of more general character. He was for many years secretary of the Société de Linguistique de Paris, and contributed almost countless articles and reviews to its *Bulletin* and *Mémoires*. He was an Honorary Member of the Linguistic Society of America from 1927; he was an Associate or Corresponding Member of the Academies of Amsterdam, Belgrade, Brussels, Copenhagen, Christiania, Dublin, Göttingen, Krakau, Leningrad, Prague, of the Philological Society of London, of the Finn-Ugrian Society of Helsinki, etc. His modest and gentle ways, and his kindly disposition endeared him to his associates. In his last five years, when his eyesight failed and his general health was impaired, he secured the cooperation of younger colleagues and kept energetically at his scientific work, as is shown by the volumes, arti-

cles, and reviews which appeared even in this period. He was elected Honorary Member of the Oriental Society in 1935. He died at Paris on September 22, 1936, aged 70.

R. G. KENT,

W. NORMAN BROWN.

The minute was accepted with a rising vote.

The Secretary presented the following minute on the death of Professor C. S. Hurgronje, on behalf of a committee consisting of Professor J. J. Obermann.

Professor CHRISTIAN SNOOK HURGRONJE died on June 20, 1936, at the age of seventy-nine.

In his passing the science of Islam has lost one of its foremost representatives in recent times. A worthy bearer of the great tradition of Oriental scholarship in his native Holland, his life-work was to become part of that tradition, adding decisively to its greatness and brilliance. With his renowned predecessors he shared prominently in the work of exploring the historic disciplines of Arab and Moslem lore. But well nigh all his own was the pioneer task of focusing the beacon of scientific research on the Arabic-Mohammedan world of today. In making his vast learning in the history and literature of the Moslems serviceable to the methodic study of their modern life and thought, he secured for his productive scholarship a unique position in the annals of Islamic Studies.

His first important contribution, *Het Mekkaansche Feest*, appeared as early as 1880. Having inquired into the nature and origin of the Hajj from literary sources, he now went to the living source. First in Jeddah and then in Mecca itself, he continued his inquiry by personal experience and first-hand observation. The result was his classical work *Mekka*, published in 1888-89, which to this day is, and will continue to be, on the list of required reading of every student of Islam. With its twofold task, this opus magnum faithfully reflects the intellectual orientation so characteristic of its author. *Die Stadt und ihre Herren* (Volume I) has remained the only scientific history of Mohammedan Mecca by a European authority. Equally unrivaled, *Aus dem Heutigen Leben* (Volume II), which appeared also in English in 1931, has given us the sole minute description of the cultural and religious life as well as the ethnic and social conditions of the Holy City of Islam as seen by the eye of a dispassionate though profoundly understanding Western scholar.

His sojourn in Mecca was universally hailed as a heroic deed of scientific exploration. Yet it was to be almost overshadowed by his exploratory achievements to come. For nearly a score of years (1889-1906) he was to stand on the outpost of contemporary Mohammedanism in Netherlands-India. In an indefatigable series of labors, he now built up, well nigh single-handedly, the structure of the science of Islam in the East Indies, the ground for which he had already laid in his *Mekka* by methodic scrutiny of the "Jawah" colony of the Holy City. His standard work of this

series is no doubt that dealing with Aceh, made available to English readers in *The Acehness*, published in 1900. It was in the same years that, upon return to his native country, he was chosen to the honor of succeeding the great *de Goeje* as Professor of Arabic at the University of Leyden.

Many members of the American Oriental Society have no doubt retained the memory of his enchanting personality from his American Lectures on the History of Religion for 1914-15, when he was made an Honorary Member of the Society; also from his Presidency of the Eighteenth International Oriental Congress at Leyden in September, 1931.

In his writings, objects of historic research are forever interwoven with current problems, the former calling for the critical judgment of the scholar, the latter challenging the views of the man, often the creed of the citizen. As an exponent of the science of Islam he was admired by all who followed his works. As a student of the Islam of today he had to taste the sting of contradiction and the bitterness of adversity. His manly protest against Turkey's proclaiming her entry into the World War as an act of *jihad*, thus stamping it the Holy War of all Moslems against the enemies of Islam, earned him the denunciation and bitterness of counter-protests even from life-long friends.

His less extensive works have been collected in the six volumes of his *Verspreide Geschriften* (1923-1927). A full bibliography of his publications, compiled by Professor A. J. Wansink, lists more than two hundred items. They form a lasting monument to the truly fabulous width of his intellect. And they will keep his name alive among students of Islam everywhere—a memory of honor and admiration.

J. J. OBERMANN.

The minute was accepted with a rising vote.

Professor W. Norman Brown presented the following minute on the death of Professor M. Winternitz on behalf of a committee consisting of Professor R. G. Kent and himself:

MORITZ WINTERNITZ, of the German University in Prague, elected honorary member of this Society in 1923, was born in Horn, Lower Austria, December 23, 1863, and died in Prague, January 9, 1937. After being admitted to the doctorate in 1886, he began his Indological career as an amanuensis to Max Müller, in Oxford, that same year. He held that position until 1890; in 1891 he took a post as teacher in the Oxford High School for Girls, at first sight an unimportant position, but it seems to have been for him the first expression of an interest in the ethnological study of women, which later expressed itself in several publications, including his work *Die Frau in den indischen Religionen* (1920). From 1895 to 1899 he was librarian of the Indian Institute at Oxford. In 1899 he went to the German University of Prague, where in 1902 he was appointed assistant professor, and in 1911 professor of Indic Philology and Ethnology. In 1934, at the age of 70, he was retired. Within this academic frame-

work he established a living body of scientific labor, publication, and encouragement of others. Of his work the greatest is his *History of Indian Literature*, first appearing in the German edition in the years 1905-08, 1913, and 1920; an English revision and translation was published in Calcutta in 1927 and 1933. Although this work overshadows his others, many of these latter are of great importance and wide range: he dealt specifically and generally with Brahmanism and Hinduism; he catalogued the Bodleian Sanskrit manuscripts; he indexed the *Sacred Books of the East*. He was interested in modern India as well as ancient, and wrote about Gandhi. For one year (1922-23) he was visiting professor at the Viśva Bhāratī University at Tagore's Sānti Niketan. He was editor of the series *Indologies Pragmatis*. His interest and enthusiasm helped materially to launch the great critical edition of the Mahābhārata now being published by the Bhandarkar Oriental Research Institute, in Poona, and to keep it going. Many Oriental societies had elected him to honorary membership. India scholarship profited greatly from his life; it suffers severely from his death.

W. NORMAN BROWN,  
R. G. KENT.

The minute was accepted with a rising vote.

#### REPORT OF THE EDITORS

Professor W. Norman Brown submitted the Report of the Editors on behalf of himself, Professor E. A. Speiser and Dr. J. K. Shryock, as follows:

During the year 1936-37 the editors have published Volume 56, parts 2, 3, and 4, and Volume 57, part 1. This is a total of 509 pages, being 85 more than were contained in the four issues reported at the last meeting of the Society. One of the issues was in honor of Professor Benstedt, and contained eighteen articles with eighteen plates, and comprised 184 pages.

The editors have also published two volumes of the American Oriental Series, namely Volume 8, *A Grammar of the Phoenician Language*, by Zellig S. Harris, and Volume 9, *The Kashmirian Atharva Veda, Books 16 and 17*, by LeRoy C. Barret. Volume 10 is now in press: *Monable Prophecy in the Nazi Documents*, by Dorothy Cross. Another volume has been accepted for the Series, *The Study of Human Abilities—The Jen Wu Chia of Len Shao*, by John K. Shryock.

During a part of the year one of the editors, Dr. Speiser, has been in Iraq excavating at Tepe Gawra, but before he left he organized his work so that the other editors could carry on until his return.

W. NORMAN BROWN,  
Editor.

The report was accepted by the Society.

Professor F. Edgerton inquired of the Editors whether they were required to refuse space in the JOURNAL to persons not members of the Society. The Editors replied that this had been the traditional practice. Upon motion of Professor Edgerton it was thereupon voted, that the Editors are not to be restricted to members of the Society in accepting manuscripts for publication in the JOURNAL.

#### REPORT OF THE LIBRARIAN

Professor A. Keogh presented his report as Librarian of the Society, as follows:

During the year 1936-37 168 volumes and 327 numbers of periodicals have been added to the Library of the Society. Of the periodicals 306 were in continuance of sets already in the Library; 21 represent titles new to the Library. Seven new titles have been added to the exchange list: *Tôhō Gakuhō*, Calcutta Oriental Journal, Harvard Journal of Asiatic studies, *Virvitāṃ*, Quarterly bulletin of Chinese bibliography, *Mémoires de la Commission orientaliste de l'Académie polonaise*, and *Catalogue des livres de la Manchourie et de la Mongolie*. Seventy-nine volumes have been forwarded to the editors of the JOURNAL for purposes of review. Through the generosity of Miss Elizabeth Dunbar the Library has this year acquired a manuscript letter of Talcott Williams and a portrait of him.

The cataloging of books, pamphlets and periodicals is up to date.

The following is the list of accessions for the year:

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- Albright, W. F. The vocalization of the Egyptian syllabic orthography. 1934. (American oriental series, v. 5)
- 'Alī Ibn Zaid al-Buhārī. *Tatimmat al-ḥikma*. Ed. by Moḥammad Shafī. 1935. 2v. (Panjab university oriental publications series, no. 20)
- All-India oriental conference. 7th, Banaru. Proceedings and transactions. 1935.
- Allen, T. G. Egyptian stelae in Field museum of natural history. 1936. (Field museum of natural history. Publ. 350. Anthropological series, v. 24, no. 1)
- Allen, W. D. 100 Chinese picture-words. [1936]
- Altekar, A. S. The *Rāṣṭrakūṭas* and their times. 1934. (Poona oriental series, no. 36)
- American institute for Persian art and archaeology. Bulletin, v. IV, no. 3. 1935.
- American philosophical association. Western division. William Torrey Harris, 1836-1935. A collection of essays. Ed. by E. L. Schaub. 1936.

- American philosophical society. *Miscellanea* v. 1, no. 2. [1935]
- , Library. Report, 1935. 1936.
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- Appadurai, A. Economic conditions in southern India (1000-1500 A.D.). 1936. 2v. (Madras university historical series, no. 12-12 bis)
- Appayya Dikṣita. *The Siddhāntaleśāsānūgraha*. With an English translation by S. S. Suryanarayana Sastri. v. I. 1935. (Publ. of the Dept. of Indian philosophy, no. 4)
- Asiatic society of Japan. Library. Catalogue. 1935.
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- Benveniste, E. *Les Infinitifs arétiqnes*. 1935.
- [Bharata] *Nāṭyaśāstra*. With the commentary of Abhinavagūpta. Ed. by M. Ramakrishna Kavi. v. 2. 1934. (Gaskell's oriental series, no. 68)
- Ehavaḥṛti. *Uttararāmacarita*, tr. et annoté par N. Stehoupak. 1935. (Collection Émile Senart)
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- Brunton, P. *A search in secret India*. [1935]
- Buitenkool, H. E. *Pārvatasiddhanta; analytisch onderzoek aangunnde het systeem der Tripaḍi van Pāṇini's Aṣṭādhyāyī*. 1934.
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- Chatterjee, J. M. *The ethical conceptions of the Gāthā*. 2nd ed. With an introduction by Dr. Bhagwandas. [1935?]
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- Chetty, T. V. G. *Om Sri Chidambaram Rāmaṅga Swamiji*. 1935.
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- Dhammapadam. The Dhammapadam translated from the Pali with an essay on Buddha and the Occident. 1936.
- Dumézil, G. *Fiamen-brahmana*. 1935. (Annales du Musée Guimet. Bibliothèque de vulgarisation, t. 61)
- Emmons, M. B. A union list of printed Indic texts and translations in American libraries. 1936. (American oriental series, v. 7)
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- Herrmann, A. Historical and commercial atlas of China. 1935. (Harvard-Yenching institute. Monograph series, v. 1)
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- — — Supplement to no. 5. *Studies in the Yün chü-ssu, P'ung-shan*. 1935.
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- Vasuputtimāra. *Tattvabindu*, with *Tattvavibhāvanā*, by Rāṣputra Paramasvara. Ed. by V. A. Ramaswami Sastri. With a foreword by Prof. S. Kappaswami Sastri and with an introductory note by Prof. K. Rama Pisharoti. [1936] (Annamalai university Sanskrit series, no. 3)
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ANDREW KINGS,  
*Librarian.*

The report was accepted by the Society.

#### REPORT OF THE AUDITORS

Professor A. Goetze presented the report of the Committee of Auditors, as follows:

It is hereby certified that we examined the accounts of the Secretary-Treasurer for the fiscal and calendar year, 1936, on January 8th, 1937, and found them correct and in accordance with the report which he has submitted to you, and which we signed at the time.

ALBRECHT GOETZE,  
ELIEN GRANT,  
*Auditing Committee.*

#### REPORT OF THE TREASURER

The Secretary-Treasurer presented his Treasurer's Report for the fiscal and calendar year 1936, as follows:

##### 1. CERTIFICATE OF HOLDINGS

December 31, 1936.

This is to certify to the fact that as of today Yale University is holding the sum of \$2,998.72 for credit of the American Oriental Society.

We are also holding the following securities in the same account:

\$6,000 mortgage on 658-60 Dixwell Avenue, New Haven, Conn.  
2,000 Morris and Essex 3½% of 2000.  
2,000 Pacific Gas and Electric 4s of 1954.  
2,000 Terminal Railroad Association of St. Louis 4s of 1935.  
2,000 Eastern Gas and Fuel Associates 4s of 1936.  
1,000 Niagara Falls Power 3½% of 1960.  
1,000 American Tel. and Tel. 3½% of 1961.  
20 sha. Chicago, Rock Island and Pacific preferred.  
10 sha. Bankers Trust Company.  
10 sha. First National Bank of Boston.  
10 sha. American Tel. and Tel.  
10 sha. Chemical Bank and Trust.

Very truly yours,

(Signed) H. J. OSTRANDER,  
*Cashier (Yale University).*

## 2. GENERAL BALANCE SHEET. ASSETS AND LIABILITIES

Assets	Book Value		Market Value
	Dec. 31, 1935	Dec. 31, 1936	Dec. 31, 1936
Investments .....	\$17,075.00	\$20,435.08	\$20,138.75
Cash Balance.....	5,780.07	2,098.72	2,098.72
Total .....	\$22,855.07	\$22,433.80	\$22,137.47
Liabilities	Dec. 31, 1935	Dec. 31, 1936	
Trust Funds.....	\$12,104.60	\$12,772.12	
Life Mem. Fund.....	4,175.00	4,200.00	
Reserve Fund.....	2,000.00	2,000.00	
Journal Balance.....	414.42	43.01	
Monograph Balance.....	4,225.42	2,976.97	
Publication of AOS X.....		110.00	
Journal Adv. Author Pymt. .		1.50	
Returnable to Offprint Authors		.50	
Com. on Research Balance...		41.00	
Debit Balance Current Funds	84.34		
	\$22,855.07	\$22,145.10	\$22,145.10
Surplus .....		1,288.70	992.37
Total .....		\$23,433.80	\$23,137.47

## 3. INVESTMENTS

		Book	Market	Yield
		Value	Value	
		Dec. 31, 1936		
Mortgage		\$ 0,000.00	\$ 0,000.00	0%
Bonds	Date of Purchase			
1 American T. and T.	Oct. 16, 1936	1,011.90	1,020.25	3½%
2 Eastern Gas and Fuel	Mar. 31, 1936	1,336.07	1,327.50	4%
2 Morris and Essex	July 9, 1936	1,000.08	1,020.00	3½%
1 Niagara Falls Power	June 25, 1936	1,031.47	1,091.25	3½%
2 Pacific Gas and Elec.	July 9, 1935	2,006.30	2,200.00	4%
2 Term. RR. Assoc. of St. L.	July 9, 1935	2,120.12	2,222.50	4%
Stocks				
10 American T. and T.	June 3, 1935	1,860.35	1,948.75	\$9.00
10 Chem. Bank and Trust	Oct. 16, 1936	667.50	615.00	\$1.80
20 Chic. R. I. and Pac.	Dec. 16, 1912	920.00	135.00	default
10 Bankers Trust	June 11, 1936	605.00	680.00	\$2.00
10 1st Natl. Bank Boston	June 11, 1936	465.00	562.50	\$2.00
Total		\$20,435.08	\$20,138.75	

4.

CAPITALIZED FUNDS

General

Charles W. Bradley.....	\$ 3,000.00
Alex F. Cothrel.....	1,500.00
William D. Whitney.....	1,000.00
I. M. Casanowicz.....	150.00
Justin E. Abbott, Dec. 31, 1935.....	\$ 6,454.56
Added to Dec. 31, 1930.....	667.56
	<hr/>
	7,122.12
	<hr/>
	\$12,772.12

Life Membership Fund

Total Dec. 31, 1935.....	4,175.00
Added to Dec. 31, 1936.....	25.00
	<hr/>

	\$ 4,200.00
Reserve Fund.....	2,000.00

5.

GROSS INCOME AND EXPENDITURES

Cash Balance December 31, 1935.....	\$ 5,780.07
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Income:

Dues.....	\$ 2,591.74
JOURNAL Sales.....	320.42
Author Charge Collections.....	191.65
Offprint Sales and Collections.....	20.79
Monograph Sales.....	309.23
Monograph Payments.....	417.57
Investments Sales.....	5,298.60
Interest on Investments.....	1,031.93
Abbott Bequest.....	667.56
Life Membership.....	25.00
Library Catalogue Sales.....	5.00
ACLS (to printing Constitution).....	20.05
ACLS (Com. on Research).....	41.00
	<hr/>
	11,069.74
Total.....	<hr/>
	\$16,849.81

Expenditures:

Secretary-Treasurer.....	\$ 1,000.00
JOURNAL (Pub. and Ed. Exp.).....	2,769.16
JOURNAL (Exp. for Authors).....	170.95
Offprint (Cost, Mgt., Refunds).....	31.10
Librarian.....	141.42
Monographs (Costs and Mgt.).....	1,895.40

Investment (purchases).....	7,307.80	
Editors' Honoraria.....	300.00	
Dues ACLS.....	25.00	
Publishing Constitution.....	30.05	
Middle West Branch (Expenses).....	100.00	
	<hr/>	13,841.09
Cash Balance December 31, 1936.....		\$2,908.72

8. JOURNAL ACCOUNT

Income:

Balance January 1, 1936.....	\$ 404.42	
Per Budget.....	2,050.00	
Sales, Yale University Press.....	\$ 327.02	
Office.....	1.50	
	<hr/>	329.42
		\$2,763.84
Author Refunds:		
Current .....	\$ 170.95	
Arrears .....	10.20	
Advance .....	1.50	
	<hr/>	191.65
ACLS, to publish Constitution.....		30.05
		<hr/>
Total.....		\$3,005.54

Expenditures:

Printing:

55, 4 Text .....	\$ 564.00	
Review copies.....	1.75	
Cuts .....	74.31	
	<hr/>	\$ 640.06
50, 1 Text .....	\$ 503.00	
Cuts .....	4.37	
	<hr/>	507.37
50, 2 Text .....	\$ 876.26	
Cuts .....	137.48	
Binding .....	2.25	
	<hr/>	1,015.99
50, 3 Text .....	\$ 530.37	
Cut .....	2.50	
Review Copies.....	1.00	
	<hr/>	542.87
Total Publication Cost.....		\$2,707.48



Editorial Expenses.....	52.71
Business Management Expenses.....	.92
Addressograph.....	3.55
Refund to Authors on Overcharge.....	4.50
Expended for Authors (JOURNAL).....	170.05
Printing Constitution.....	30.05
<b>Total Expenditure.....</b>	<b>\$2,970.16</b>
Income.....	\$3,005.54
Expenditure.....	2,970.16
<b>Remainder.....</b>	<b>\$ 35.38</b>
Liability on Adv. Auth. Refund.....	1.50
<b>Offprint. Balances.....</b>	<b>\$ 33.89</b>
<b>Balance Dec. 31, 1936.....</b>	<b>\$ 43.01</b>

7. MONOGRAPH ACCOUNT

*Income:*

Balance January 1, 1936.....	\$4,255.43
Sales less expenses to Dec. 31, 1935 (Speiser).....	\$ 71.53
Sales less expenses to Dec. 31, 1935 (Yale Press).....	42.07
Royalties (Yale Press).....	9.10
Sales less expenses to April 18, 1936 (Speiser).....	43.41
Sales less expenses to June 3, 1936 (Speiser).....	15.27
<b>Sales to Dec. 31, 1936 (Kraeling).....</b>	<b>194.39</b>
<b>Sales to Dec. 31, 1936 (Kraeling).....</b>	<b>124.85</b>
Payments on account:	
AOS VIII (Harris).....	\$ 230.87
AOS IX (Barret).....	67.90
AOS X (Cross).....	110.00
<b>Total.....</b>	<b>417.77</b>
<b>Total.....</b>	<b>\$4,982.43</b>

*Expenditures:*

Express (on AOS V, VI, VII, VIII, IX).....	\$ 40.20
Supplies (Speiser).....	19.50
Supplies (Kraeling).....	9.04
Postage.....	14.41
Advertising.....	47.55
Transfer to Brown (AOS VII).....	8.48
Reader's Fee (AOS X).....	10.00
Editor's Fees (AOS VIII, IX).....	100.00

Costs AOS VIII.....	800.26	
Costs AOS IX.....	770.42	
	<hr/>	1,565.40
Remainder .....		\$3,095.97
Liability (to Publication of AOS X).....		116.00
		<hr/>
Balance Dec. 31, 1930.....		\$2,979.97
Outstanding:		
Sales payments overdue (Speiser).....	\$ 23.97	
Sales payments overdue (Kracling).....	9.07	
Current outstanding items.....	67.43	
	<hr/>	\$ 80.29

## 8. SECRETARY-TREASURER'S ACCOUNT

Income from Budget.....		\$1,000.00
Expenditures:		
Archer-Barret to May 0.....	\$ 220.68	
Clerical Assistance.....	322.25	
Addressograph Roll.....	20.25	
Stationery, Supplies.....	169.19	
Travel .....	30.00	
Postage, Telegrams, Express.....	25.78	
	<hr/>	\$ 707.15
Honorarium .....	202.85	
	<hr/>	\$1,000.00

## 9. OFFPRINT ACCOUNT

Income:		
Author Payment (No. 7).....	\$ 7.35	
Sales Balance (Speiser).....	0.49	
Sales .....	6.05	
	<hr/>	\$20.79
Expenditures:		
Manufacture (Offprint 7).....	\$ 7.35	
Postage .....	.11	
Refunds to Authors.....	3.70	
	<hr/>	11.16
		<hr/>
Remainder .....		\$ 9.63
Transferred to JOURNAL Account.....		0.13
		<hr/>
Balance (Due to Authors).....		\$ 9.60

10. LIBRARIAN

*Income:*

Per Budget.....	\$100.00
Sale of Catalogues.....	8.00
	<hr/>
	\$108.00

*Expenditure:*

Binding .....	\$ 77.05
Purchase of Books.....	64.37
	<hr/>
	141.42

Overdraft .....	\$ 36.42
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11. BUDGETARY ASSIGNMENTS AND EXPENDITURES 1936

<i>Estimated Income</i>		<i>Actual Income</i>
(Budget for 1936)		(Same items)
Dues .....	\$2,400.00	\$2,501.74
Interest .....	950.00	1,031.93
	<hr/>	<hr/>
	\$3,350.00	\$3,533.67
Needed to Balance.....	149.34	
	<hr/>	
Total.....	\$3,509.34	
 <i>Authorized Expenditure</i>		 <i>Actual Expenditure</i>
(Budget for 1936)		(Same items)
JOURNAL .....	\$2,050.00	\$2,050.00
Ed. honoraria.....	300.00	300.00
Secretary-Treasurer .....	1,000.00	1,000.00
Dues ACLS.....	25.00	25.00
Middle West Branch.....	100.00	100.00
Encyclopedia of Islam.....	50.00	
Librarian .....	100.00	130.42
Current Funds Debt (1935).....	84.34	
	<hr/>	<hr/>
Total.....	\$3,509.34	\$3,611.42

Respectfully submitted,

CARL H. KRAELING,  
*Secretary-Treasurer.*

The Treasurer's Report as audited was accepted.

REPORT OF THE EXECUTIVE COMMITTEE

The Secretary reported on the actions of the Executive Committee as follows:

During the past year the Executive Committee has functioned on behalf of the Society, taking actions necessary to the Society's welfare in ten mail votes. Most of these concerned the election of new members, whose names have already been reported in the JOURNAL. The following names are reported herewith:

Sheikh Yaseen al-Abbasi  
 Basil M. Alexiev  
 Henry E. Allen  
 William M. Austin  
 Robert W. Barnett  
 V. I. Basanoff  
 Marybelle Bouchard  
 Denzel Carr  
 W. T. Chen  
 Mrs. Catherine E. B. Cox  
 George H. Cressey  
 Elmer H. Cutts  
 J. J. L. Dayvendale  
 Ruth Karachaw  
 John F. Emling  
 Charles B. Fabe  
 Charles M. Fleischner  
 George B. Fowler  
 Merton B. French  
 Charles T. Fritsch  
 Richard B. Fuller  
 Peter S. Gaerts  
 Hetty Goldman  
 Godfrey Goossens  
 Thomas W. Graham  
 Louis Hartman  
 A. Eustace Haydon  
 Bernard Heller  
 J. Howard Howson  
 Forrest R. Holdtammer  
 Witold Jalsowski  
 William C. Johnston  
 Edward S. King  
 Edward A. Kracke, Jr.  
 T. W. Kreischmann  
 Thomas La Fargue  
 Silva Lake  
 K. C. Lechrick

S. H. Leger  
 F. D. Lessing  
 Edwin C. Lobenstine  
 Albert P. Ludwig  
 David W. Lyon  
 Hardin T. McClelland  
 Lewis A. Maverick  
 Yutaka Minakuchi  
 Charles A. Moore  
 William L. Newton  
 Wallace C. Paul  
 Maurice B. Pekarsky  
 Southwick Phelps  
 Malcolm Pitt  
 F. L. H. Polts  
 Ernest B. Price  
 Earl H. Pritchard  
 Shunzo Sakamaki  
 Eric Schroeder  
 Benjamin Schwartz  
 Mrs. Kate C. Seelye  
 Mrs. Abbie E. Sherman  
 Lawrence C. S. Sickman  
 Sir Jogendra Singh  
 Hortense Spahr  
 Mrs. Susan R. Stiller  
 William F. Stinespring  
 Rufus Suter  
 Henry S. Tatsumi  
 Ryusaku Tsunoda  
 Joachim Wach  
 Friedrich Weller  
 Tu-Chien Weng  
 W. E. Wheeler  
 H. R. Williamson  
 Joseph K. Yamagiwa  
 Chitoshi Yamaga  
 T. L. Yuan

Not all of these have as yet qualified for membership.

The Executive Committee met yesterday evening, March 30, for the transaction of further business.

It received a full report of the work of the Membership Committee and voted to record its appreciation of the Committee's excellent work.

It voted to elect to corporate membership, subject to their qualification for membership, the following 23 persons:

Joseph L. Baron	Hugh B. MacLean
Stanley Boggs	Mrs. W. S. Meek
Holmurt de Terra	Carl Raswan
Darley Downe	Richard C. Rudolph
Ethel C. Effkins	Alfred von R. Sauer
Harry Fee	Lewis V. Thomas
Norman R. Gentry	Vivian Totten
Y. Ichikushi	Reuben S. Turner
Stillson J. Judah	Florence Walne
Marcus Kramer	Samuel T. Wilson
Paul M. A. Lincharger	Helen Zimmerman
Lindsay B. Longene	

It voted to retain on the membership rolls until Dec. 31, 1937, the following persons already two years in arrears: T. T. Shui, Sir D. Baron Jayatilaka.

It voted to admit to Life Membership Ellen S. Ogden, a member of the Society since 1895.

It voted to nominate to the Society for election to Honorary Membership the three distinguished scholars whose names are to be presented to the Society by the Chairman of the Committee on Honorary Members after the reading of this report.

It received from Professor H. H. Bender the report of the Committee on the Promotion of Oriental Research which has given effective assistance during the year to the publication of manuscripts of outstanding merit.

It received detailed reports of the Secretary-Treasurer on the management of the Society's affairs and its finances during the past year.

It was voted to adopt the following budget for the calendar year 1937:

AMERICAN ORIENTAL SOCIETY BUDGETARY ASSIGNMENTS, CALENDAR  
YEAR, 1937

General Surplus and Unassigned Income:

General Surplus, Dec. 31, 1935.....	\$1,288.70	\$ 932.37*
Estimated Unassigned Income, 1937:		
Dues .....	\$2,950.00	
Interest on Cash.....	100.00	
Interest on Investments.....	904.25	
	<hr/>	<hr/>
	3,954.25	3,954.25
Total Available for Assignment.....	\$5,242.95	\$4,940.62

\* The second figure represents the General Surplus determined by the market value of the securities held as of Dec. 31, 1935, while the first is determined by the book value of the same securities.

## Budgetary Assignments:

JOURNAL Printing.....	\$2,185.17		
JOURNAL Mailing.....	150.00		
Editors' Honoraria.....	400.00		
Secretary-Treasurers Account.....	1,000.00		
Committee on Research.....	50.00		
Committee on Resources.....	200.00		
Committee on Membership.....	150.00		
Librarian's Account.....	200.00		
Middle West Branch.....	100.00		
ACLS .....	25.00		
Total Assignments.....	\$4,440.17	4,440.17	4,440.17
Remaining Unassigned Surplus.....		\$ 302.78	\$ 506.45

## Explanation of JOURNAL Budget Figures:

Balances Dec. 31, 1936:

JOURNAL Balance.....		\$ 49.01
Advance Author Payment.....		1.50
Requested for 1937.....	\$2,000.00	
Assigned Income (JOURNAL Sales)....	434.83	434.83
To be budgeted.....	\$2,165.17	2,165.17
Total Available for JOURNAL Publication.....		\$2,644.51

It was voted that the President appoint a committee to endeavor to collect archival material bearing upon the history of the Society.

It was voted that the President appoint a committee to study the suggestion presented by Professor T. J. Meek concerning the disposal of the Society's Library.

It was voted to appoint Professor F. Edgerton chairman of the Committee for the Enlargement of Resources.

It was voted to reappoint Professors LeRoy C. Barret and Ira W. Price as the Committee on Investments.

It was voted to appoint Professor A. H. Lybyer chairman of the Committee on Membership.

It was voted to reappoint Dr. A. W. Hummel as a member of the Committee on the Promotion of Oriental Research.

It was voted to recommend to the incoming Executive Committee the appointment of a committee to consider and plan for the celebration of the Society's anniversary in 1942.

The report of the Executive Committee was received by the Society.

### ELECTION OF HONORARY MEMBERS

On behalf of the Committee on Honorary Membership, consisting of Professors W. N. BROWN, J. A. MONTGOMERY, and R. G. KENT, Professor Brown presented the following nominations for honorary membership:

DAVID SAMUEL MARSHLOUTH, M.A., D.Litt., Laudian Professor of Arabic, University of Oxford, since 1880. Born October 17, 1858. Author of numerous publications, mostly connected with Arabic literature. Address: Kenney, Bear's Hill, Oxford, England.

STEN KOSOW, Professor of Indic Philology at the University of Oslo since 1910, with an interruption from 1914-1919, when he was at the University of Hamburg. Born April 17, 1868. Author of numerous publications dealing with Indic languages, literature, inscriptions, religion, and history. Editor of *Acta Orientalia*, formerly editor of *Epigraphia Indica*. Address: Ethnographic Museum, Oslo, Norway.

HANNS OERTZ, sometime Professor of Indic Philology, University of Munich. Born April 20, 1868. Author of works on Linguistics and Indic languages, literature and religion. Address: Pienzenauer Str. 36, München, Germany.

The nominees were elected, each by a rising vote.

### REPORT OF DELEGATES TO THE AMERICAN COUNCIL OF LEARNED SOCIETIES

Professor F. Edgerton reported on behalf of the Delegates to the American Council of Learned Societies as follows:

The Council met in New York City, in the Montclair Hotel, on January 29 and 30, 1927. In addition to the delegates, the following members of the American Oriental Society were also present: Messrs. Albright, Starbuck, Kraeling, Brown, Kent, Fligh, Hammel, Warner, Clark, and Cadbury.

A minute was read on the death of Dr. H. B. Meyer, of the Library of Congress. The American Numismatic Society was admitted to representation on the Council. An amendment to the By-Laws was passed, creating the office of Comptroller, and defining its duties. It was decided to appoint a Committee on Public Relations, and that the offices of the Council should remain in Washington.

The Council now has at its disposal no free funds for major projects of the sort which it has carried on in the past. It has, however, several large funds donated for special purposes by various foundations. The most important of these are the two annual grants of \$20,000 each for publication of works of scholarly merit, one in the field of the Fine Arts, and one in the humanities at large; and two of \$15,000 each, one of which is to be used for "planning and development" of scholarly work in humanistic lines now comparatively neglected in this country, and the other for

"study aids," pre-doctoral or post-doctoral fellowships, designed primarily to train competent scholars in such neglected fields. The Council's special Committees, several of which will presently be mentioned, illustrate the fact that certain departments of oriental studies are prominently included among the fields in which development is planned by the Council. The fund for publication should also be of interest to many of our members.

There were a number of matters of especial interest to the members of the American Oriental Society. The report of the meeting of the Union Academique Internationale gave an account of the difficulties of international projects at the present time, and of the welcome extended to the representatives of the Austrian and German academies. The Committee on Chinese Studies reported progress in a number of undertakings, and particularly on the translation of the History of the Western Huns by Dr. H. H. Dubs. The Committee on Japanese Studies will be reorganized, with Mr. Robert Hetschauer, of Princeton University, as chairman. The Committee on Indic and Iranian Studies reported that it has not yet been possible to organize a school in India, but that important excavations have been made in the Indus Valley, and that Dr. Murray Emeneau was continuing his researches into Dravidian languages. Three new committees were formed, of which two will be particularly interesting to the Oriental Society. The first is the Committee on Mediterranean and Near Eastern Studies, and the second is the Committee on the History of Religion.

Some time ago there existed an American Committee for Lectures on the History of Religions, under whose auspices lectures were given, and published, over a period from 1894 to 1923. The lecturers were distinguished scholars, and produced a number of important books. What was left of this committee has now turned over a fund of more than \$6,600.00 to the Council, which will administer it through a new committee of which Professor Neck, of Harvard University, will be the chairman.

The *Dictionary of American Biography* has been completed, but the project will be continued. It was decided to continue the *Linguistic Atlas*, and to plan for it to cover the United States and Canada. At the dinner of the Council on Friday evening, the speakers were President Keppel of the Carnegie Corporation and Secretary Moe of the Guggenheim Foundation.

FRANKLIN ROBERTSON,  
J. K. SERTOFF.

The report was accepted.

#### REPORT OF THE REPRESENTATIVE TO THE AMERICAN SCHOOLS OF ORIENTAL RESEARCH

The Secretary-Treasurer read the following as the report of Professor N. Schmidt, Representative of the Society on the Corporation of the American Schools of Oriental Research:

As your representative on the Board of Trustees of the American Schools of Oriental Research I have the honor to present the following report.



When the long and distinguished service of Professor W. F. Albright as Director of the School of Jerusalem came to an end, Professor Nelson Glueck was appointed as his successor. The new Director, trained by a master of Palestinian archaeology and well fitted for the position, is carrying on the work with marked success. Professor W. C. Graham of the University of Chicago is the Annual Professor for 1936-37, and Dr. Clarence S. Fisher is Professor of Archaeology. Several visiting scholars are serving as Honorary Lecturers. The three Fellowships are held by men of ability; and a number of students are in attendance. Beside the daily lectures, there are frequent excursions. Recently trips have been made to Syria and to Transjordan. Minor excavations have been undertaken. The school building is in excellent condition; the hostel is well filled and wisely managed. By purchases and gifts the library is steadily increased, and there is a small museum, with exhibits carefully arranged. It is gratifying to record the hearty co-operation with the Palestine Oriental Society, the British School of Archaeology, the Dominican Ecole Biblique, and the Hebrew University, as well as the cordial relations with the Christian, Jewish, and Muslim elements of the population. Professor S. Vernon McCasland of Goucher College has been appointed Annual Professor for 1937-38.

Professor E. A. Speiser of the University of Pennsylvania is Director of the School in Baghdad. Professor A. T. Olmstead of the Oriental Institute of Chicago is Annual Professor for 1936-37; Mr. E. Bortow Müller serves as architect. The Director is in charge of the important excavations at Tepe Gawra and Tell-Billa. The greatest need of the school is a house of its own to provide a centre of its activities, a home for professors, students, and visiting scholars, and suitable rooms for public lectures, a library, and a museum. This would increase the facilities for study and research in Iraq, and enhance the dignity and prestige of the institution. One or more fellowships would be desirable. Professor Elihu Grant of Haverford College has been appointed Annual Professor for 1937-38.

In the forthcoming volume of the *Annuaire* a large number of texts from Nuzi will be published. The *Bulletin*, appearing regularly four times a year, presents news and notes of archaeological interest and interpretations of fresh discoveries and recent researches by the indefatigable editor, Professor Albright, and other eminent scholars. Of Professor Fisher's monumental *Corpus of Palestinian Pottery* the first volume is now completed, and a second volume of Professor Speiser's *Tepe Gawra* is in preparation. Articles by experts connected with the schools are appearing constantly in scientific journals.

Thanks to the appropriation of the Rockefeller Foundation and a large gift by Mr. and Mrs. Felix M. Warburg, the financial condition of the schools has been greatly improved. In order, however, to maintain the existing standards and to guarantee a sound development it is of utmost importance that a more adequate endowment be secured. This has been made possible by a generous offer of the Rockefeller Foundation which has pledged a substantial sum for the purpose with the understanding that it

will appropriate two dollars toward the stipulated amount for every dollar contributed from other sources before the end of 1939. The trustees are seriously considering the most effective methods of raising the needed money. Many contributions will be necessary and the time is short. A modest appropriation by our Society, as one of the parent organizations, would be a gracious gesture. With earnest efforts on the part of local groups and the recently formed association of alumni, and with special appeals to persons known to be interested in Palestinian archaeology and Oriental research, it should not be beyond our powers to meet the present opportunity. There is every reason for looking with confidence to the future.

NATHANIEL SCHMIDT.

The report was accepted.

The Chairman appointed Professors C. J. Ogden, W. H. Worrell and L. C. Barret a Committee on Resolutions to report at the Sixth Session.

The Secretary-Treasurer was instructed to send telegrams to Professor C. R. Lanman conveying the Society's greetings, and to Professor and Mrs. A. V. W. Jackson reciprocating their greetings, and expressing to all our regret of their inability to participate in the meeting.

The business meeting adjourned at 11.40 A. M. After an intermission of 10 minutes Professor B. H. Sturtevant read his Presidential Address, "Analogical Creation and Contamination as Illustrated by Lapses" (see this issue of the JOURNAL, pp. 135 ff.).

## SECOND SESSION

The second session was called to order by President Sturtevant in the Main Lecture Room of the Cleveland Public Library at 2.25 P. M. on the same day. The following communications were presented:

Professor O. R. SEIDERS (Presbyterian Theological Seminary, Chicago; President of the Middle West Branch): Social Implications of Solomon's Accession.

The coup which put Solomon on the throne is best understood by considering the three leading characters in carrying it out: Nathan, Zadok, and Bath-sheba. It established the hereditary kingship, but disregarded the principle of primogeniture. It was a blow to the authority of the elders and the centralization of government in a royal dictatorship, with large standing army, high taxation, complete government control of the individual and power to liquidate. It meant the concentration of wealth in the hands of the few and the impoverish-

ment of the masses. Solomon's reputation as a man of peace is due in a measure to a late interpretation of his name.

GERSON W. THAYER (Cleveland Public Library): The John G. White Collection of Orientalia.

The John G. White Collection of Folk-lore, Orientalia, and Chess is on the third floor of the Cleveland Public Library, and is open weekdays from 9 to 6 o'clock. Its 75,000 volumes include some of the most important Oriental collections in the country. In honor of the Society, it will show a number of exhibits, including Tibetan books, Chinese Mohammedan literature, and Arabic and Persian manuscripts.

Dr. MUHAMMED A. SIMSAR (University of Pennsylvania): New Light on Zâ'if, a 16th Century Turkish Poet.

PIR MUHAMMED Zâ'if is a Turkish poet about whom there is practically no information. Hajji Khalifah, the Turkish encyclopaedist, states only that Zâ'if made a translation into Turkish of Sa'di's Gulistan. No more information is to be found about his life or other works elsewhere. The writer has located a rare manuscript containing copies of two of his works. By study of this manuscript he has brought to light a good many interesting facts about the poet.

Professor N. J. ROST (Dropsie College): An Ancient Egyptian Register of Birth.

The paper deals with a Demotic Register of Birth, preserved in Turin (Italy) which has been misinterpreted by previous editors.

Professor ALBERT GOETZE (Yale University): The Tense-System of the Ras-Shamra Language.

The usage of the so-called perfect (theme *qatata*) and the so-called imperfect (theme *yaqtulu*) is described and the inference drawn that none of them covers the sphere of a present-future. It is shown that there exists with the force of a present-future the theme *paqatata*. In spite of the fact that the vowels are not written its existence can be proved by several sets of facts. The tense-system of the Ras Shamra Language, it is concluded, is entirely different from that of all known West Semitic languages. This is a very strong argument (among others) against those who take the Ras Shamra language as an early Canaanite.

Dr. H. L. GINSBERG (Jewish Theological Seminary, New York): The Dating of the Eshmunazarid Inscriptions.

Clermont-Ganneau's Ptolemaic dating is right against the prevailing late Persian one. The *da mlka* of the Eshmunazar inscription, standing in three others in apposition to *ptmgya*%, is the exact equivalent of *šbr*šr *bašleš*š; for the Abiram inscription—and the Ugarit texts—have revealed that the Phoenician word for "kingdom" is *mlk* (whereas *maxlakt* invariably means "prince"). Further, *tašt šmš* (Tahiti, Eshmunazar) is a translation-borrowing of *špš* *ššš*; for

in the Semitic field it recurs only in Ecclesiastes, which is certainly Ptolemaic. Conclusion: Tabnit's reign and Eschmunazar's accession possibly antedate 320 B. C.; Eschmunazar's death and Bodaslitari's reign fall between 320 and 280.

Professor LEROY WATERMAN (University of Michigan): *Some Determining Factors in the Northward Progress of Levi.*

Many obscurities gather around the sons of Levi, nevertheless their relation to Yahweh and to Moses assures our lasting interest in them. Analysis of the Levitical genealogies has opened up unsuspected vistas in the early organization of the Levites. The earliest grouping thus obtainable is datable before the rise of David, and reveals the Levites established in Judah, and made up of local town priesthoods and the original followers of Moses. The analysis makes clear how the Levites became a part of later Judah and subsequently spread to Israel. The primary genealogy furnishes no grounds for thinking that the Levites were ever in Egypt.

Dr. SAMUEL FRIEDL (Oriental Institute): "The Humble One Riding on a Donkey" and its Babylonian Parallels.

The phrase "the humble one riding on a donkey" (Zachariah 9: 9 f.), which is part of the Jewish eschatological view, is traced to nine Babylonian liver omens. The Babylonian expressions are investigated. The study has bearings on the history of the messianic idea in Israel.

### THIRD SESSION

The third session was called to order by Vice-President Waterman in the Salle Moderne of the Hotel Statler at 8.10 P. M. on the evening of the same day. The following communications were presented:

Mr. G. EUGENE WATSON: *Palestine in the Chalcolithic Age.*

Recent excavations in Palestine and Syria have illuminated the fourth millennium to an extent hardly conceivable a decade ago. In Palestine the relative chronology of cultures is becoming fixed on stratigraphical and typological grounds, and the absolute chronology is becoming more definite through traceable cultural interchange with Mesopotamia, Syria, and especially Egypt. The center of settled community life seems to have been in the Jordan Valley, from thence moving into the Plain of Esdraelon with the settlements of Megiddo and Beth-shean, and finally entering the hill country with the founding of such cities as Ai, Jerusalem, and Gazer (cir. 32nd-31st cents. B. C.).

President JULIAN MOSENSTERN (Hebrew Union College): *The Evolution of the Figure of Satan in the Old Testament.*

Beginning with an interpretation of the role of Satan in the scene depicted in Zech. 3, this paper endeavors to trace the successive stages

of the evolution of both this scene and this figure backwards to their earliest antecedents in biblical literature, and then onwards through later references to both scene and figure in O. T. writings.

Professor MORRIS HORNKAMMEN (Hebrew Union College): Deutero-Isaiah's Attitude toward Cyrus (prefaced by a Reference to his Exilic Psalms).

This paper deals with the twofold problem of Isaiah 40-55. First, although Deutero-Isaiah describes the masses as still blind and sinful, he is so convinced that Israel's deliverance is imminent that he declares: God will blot out their transgressions. This conviction presents a departure from the belief of the pre-exilic prophets, and from the belief he himself expressed in his exilic Psalms, that redemption was contingent upon the people's spiritual awakening. Second, why did Deutero-Isaiah not judge Cyrus' conquests as Isaiah judged the conquests of Tiglath-Pileser III and his successors? Why did he hail Cyrus as the signal subject of divine favor?

Professor MORTON S. KYSLIN (Crozer Theological Seminary): The Date of Peter's Confession.

The purpose of this paper is to raise two questions: First, Is not the story of the Transfiguration based upon a tradition of the resurrection appearance to Peter referred to in Luke 24: 34 and I Cor. 15: 5? Second, Is not the story of Peter's famous confession at Caesarea Philippi the rewriting of an older story of the way Peter, once he had "turned again," had "established his brethren?"

Professor K. C. EVANS (Trinity College, Toronto): Geographical Allusions in Josephus' *Antiquities* XIV, §§ 158-488 (*Jewish War* I, §§ 203-357).

I, 203-357 displays a number of cases of vagueness or inaccuracy. In the corresponding section in the "*Antiquities*" (XIV, 158-488) the writer has carefully improved the phraseology, at the same time preserving to a remarkable degree the language employed in the "*War*." The bearing of this study on Thackeray's "Fatigue-Theory" (*Josephus*, pp. 105 ff.), on the theory of Deissner (*Die Quellen*, pp. 12 ff.), rejected by Laqueur (*Der jüdische Historiker*, pp. 123 ff.), that this section of the *War* represents an abridgment of the original, and on the question of Josephus' employment of amanuenses.

Professor W. E. STAPLES (Victoria University, Toronto): The Tetragrammaton as Pipi.

Since the form Pipi seems to have arisen about the same time as the translation of Aquila in which the Divine Name appears in archaic letters, the Greek form of the Tetragrammaton cannot have been the result of its similarity in appearance to its Hebrew form written in square characters. The Greek form, on the other hand, has certain virtues in itself which render it an extraordinarily apt rendering of the Hebrew Divine Name.

## FOURTH SESSION

At the fourth session the Society divided into three groups for the presentation and discussion of papers in special fields.

The Near Eastern Group was convened at 9.00 A. M. on April 1, in the Salle Moderne of the Hotel Statler, with Vice-President Waterman presiding. The following papers were read and discussed:

Dr. CHARLES FRANÇOIS NIMS (Oriental Institute): Some Notes on the Family of Mereruka.

Various theories on the position and relationship of the "king's daughter" Watetkhether and her son, the "king's son" Merytet, shown in the mastaba of Mereruka at Sakkarah, have appeared in print. During the work of the Oriental Institute at this tomb it was possible to give careful consideration to the inscriptions, and particularly to the recutting of the names in that part of the mastaba belonging to Merytet, and it becomes necessary to reconsider the relationships in the light of new evidence. It appears probable that Merytet was not the son of a king, as has been claimed, but was actually the son of Mereruka.

Professor W. H. WANKEL (University of Michigan): Popular Traditions of the Coptic Language.

From the time when Coptic ceased to be commonly spoken down almost to the present, there has existed a school language and a Coptic-Arabic jargon. These are not entirely derived from sacred texts, but have traditional contact with living Coptic. Greek letters were not always taken over with their Greek values. The original values of eta and epsilon must be reconsidered. Modern peasant tradition is not so corrupt and arbitrary as we have supposed.

Dr. CRAIG H. GORDON (Johns Hopkins University): Additions to Hurrian Lexicography.

The Hurrians were an important ethnic group in the Near East especially during the second millennium B. C. Thanks to recently published material from Boghazkoi, Ras Shamra, and Nuzi, considerable progress is now being made in deciphering the Hurrian language. The Nuzi tablets, though written in Middle Babylonian, are replete with Hurrian loanwords. A list of 114 of these words is available in BASOR, no. 64, pp. 23-28. The present paper consists of additions to this list. These added words will bring our Hurrian vocabulary closer to the minimum needed before we can interpret Hurrian texts with any certainty.

Professor THÉOPHILE J. MEER (University of Toronto): The Element *i-ti* in Old Akkadian Personal Names.

Ungnad interpreted *i-ti* as the preposition "with," but this in Old Akkadian is *it-ta*. Lewy and Thureau-Dangin have equated *i-ti* with Babylonian *it-ti*, but the equivalent of the latter in Old Akkadian is *i-da* (*it-ta*), just as *it-ma* is of the later *it-ma*. Old Akkadian *i-ti* is rather to be equated with Old Assyrian *i-ti* (var., *i-ti*) and both are to be equated with the later *iddin* indicated among other things by a name like *I-ti-S-e* alongside of *I-ti-in-S-e*. Furthermore, if *i-ti* is not to be derived from *uadinnu*, there remains nothing in the personal names of Old Akkadian to correspond to the element *iddin*, so common in personal names of other periods. Other reasons for the derivation are given.

Mr. A. SACHS: Unrecognized Uses of Two Cuneiform Ideograms.

Personal names from the Late Assyrian period, the first element of which has hitherto been read *Ellu-bab-* (or *Istar-bab-*) are to be read *Kù-būba-*, i. e., the divinity *Kubaba*. In Tunnach Letter No. 2, l. 10 read *GI(!)KAK.G.TAG.GA.URUDU(!)* "copper arrows" instead of the former readings *pa-ni u-dag-ga-al* or *zi-ni u-dag-ga-al* (*Hrozni*). Read Amarna Letter No. 287, l. 7: *URUDUKAK(!)G.TAG.GA* instead of Kundtson's *ard Kak ū-tug-ga*.

Dr. HORACE ABRAM RICE, Jr. (Harvard University): The Route of Sargon's "Eighth Campaign."

In this paper there are shown first, the impracticability of the routes already suggested and their topographical inconsistencies, and second, that Sargon almost certainly never went around Lake Van and very probably did not go around Lake Urmiah. An attempt is then made to show a more probable route which lay more southward and eastward than has heretofore been assumed. Certain countries and cities are tentatively located, among them being Uesi, which is placed somewhat south and east of Lake Van.

Professor IRA M. PRICE (University of Chicago): H. de Genouillac on "Lagash" and "Girsu."

H. de Genouillac excavated in the ruins of Tellah three seasons, 1929-31. On the basis of his own finds, with those of his predecessors, de Sarzec and Cros, on the same areas, he concludes that Lagash was the country and Girsu was the city in that country. An examination of de Genouillac's methods and means employed in reaching that conclusion and their validity is the purpose of this paper.

Professor MILLAR BURNOWS (Yale University): The Complaint of Lahan's Daughters (Gen. 31: 14-16).

Comparison with extra-biblical sources, particularly a remarkable series of parallels in the Nuzi tablets, affords a new insight into the meaning of this passage and its significance for the nature and history of Israelite marriage.

## INDO-EUROPEAN GROUP

The Indo-European Group was convened at 9.30 A. M. on April 1 in the Yellow Room of the Hotel Statler with President Sturtevant presiding. The following papers were read and discussed:

Professor CHARLES S. BRADEN (Northwestern University): *Hinduism and the Outcastes.*

Professor LEROY C. BARRETT (Trinity College): *The Kashmirian Atharva Veda, Book Eighteen.*

This paper presents in brief form a statement of the contents of this book, particularly as related to the third division of the text of the Atharva Veda according to the Śaunakins.

Dr. ANANDA K. COSMIKASWAMY (Boston Museum of Fine Arts): *The Indian Doctrine of Man's Last End.*

The question is asked, What distinction, if any, can be made between the Indian concept of deification and that of such Christian authors as St. Bernard or Meister Eckhart. The principle is accepted as common to both doctrines, and as unimpeachable, that a distinction of creature from Creator, finite from Infinite, is absolute. Therefore, as Eckhart expresses it, "The soul must put itself to death": in Indian terms, liberation is from soul and body (*nāma-rūpa*, *sarifiedāna śarira*). Anonymity is a fundamental condition of return to God, "Who has not become anyone," the conclusion inevitably following that no one can return to Him in likeness of nature who still is anyone. Man is compounded of body, soul, and spirit: *rūpa*, *nāma*, *ātman*. It is only the spiritual or pneumatic essence that can be thought of as returnable into its source "as milk might be poured into milk": as St. Paul expressed it, "Whoever is joined unto the Lord is one Spirit."

Professor FRANKLIN EDGERTON (Yale University): *The Gerund in Buddhist Hybrid Sanskrit.*

The Sanskrit gerund suffixes *teṣ* and *ya* are used nearly as in Classical Sanskrit, that is, *teṣ* with simple and *ya* with compound roots (but with metrical variants *tes* and *yā* respectively). However, in addition the dialect uses several Prakritic gerund endings: *iteṣ* (metrical variant \*a; also known to Sanskrit), *ipa* (*ipā*), (*i*)*teṣa* (metrical variants \*ana, \*ana), *ipana*, and *i* (*i*). The last has been noted only in Apabhraṃśa; the others are found in Pali and (or) Ardhamāgadhī. These endings are used indiscriminately with simple and compound roots. They are, however, attached not to the "root" of Sanskrit, but to a thematic present stem itself often foreign to Sanskrit, but used in Buddhist Sanskrit, Pali, or Prakrit), minus the stem-final *a*. This same thematic present stem functions extensively in this dialect as base for other verb forms (such as aorists, futures, infinitives, past passive participle)—in fact for the whole conjugation except the perfect, which is a strictly Sanskrit form, unknown to any Prakrit aside from



a few isolated forms and archaisms. There are, finally, traces of the Prakritic ending *tā* {of Ardhamāgadhī, = Skt. *taḥ*}, appearing here metri causa as *tā*.

Professor PAUL E. DUMONT (Johns Hopkins University): A Note on *Taittiriya-Saṃhitā* IV, 7. 13. m.

*id budhyanvāṅma prāti jagghy enam  
iṣṭāpūrte sām arjethām apām ca*

This text has been translated in different ways by Griffith, Eggeling, Culand, and A. B. Keith; none of these four translations can be considered as satisfactory. Another interpretation seems to be more appropriate.

Professor GEORGE DOMINIKOV (University of Chicago): The Sacred Thread, a Possible Survival of a Ritual Custom of the Indus Valley Civilization.

Tilak in his *Orion*, and recently Altkar in *JBOBS*, XX, pp. 136-139, argue that the term *yajñopaveita* originally referred to the wearing of the upper garment in a certain way, and not merely to the "sacred thread." The earliest Sanskrit passages dealing with this matter are not altogether clear, but later authorities seem to favor the "sacred thread" interpretation. However, attention may be called to a statue from Mohenjo Daro, described by Mackay as a "meditating yogi." The robe worn by this statue is carried over the left shoulder and under the right arm, that is in the precise position of the *yajñopaveita*. Definite conclusions are hazardous, but perhaps we may see here the ancestor of the *yajñopaveita*.

### FAR EASTERN GROUP

The Far Eastern Group met at the Cleveland Art Museum and was convened at 9.25 A. M. on April 1, with Prof. K. S. Latourette presiding. The following papers were read and discussed:

Mr. C. MARVIN WILSON (Field Museum, Chicago): Economic Aspects of Slavery During the Han.

Percentage of total population. Sources of slaves: families of major criminals, prisoners of war (?), kidnapping, sale or self-sale, pawning, trade with marginal peoples, transfer between government and private owners. The slave trade: markets, price, transport. Economic uses of slaves: 1. Government—palace servants, bureau clerks, factories, parks and ranches, grain transport; revenue from sale, revenue from special taxation. 2. Private—sumptuary, retainers, "manufacturing" and trade, agriculture (?). Slave holding as a criterion of wealth. Concluding remarks: related social and legal phenomena. Economic importance of system.

MISS GUSSIE ESTHER GASKILL (Cornell University): On Certain Foreigners who assisted in the Suppression of the Taip'ing Rebellion in Chekiang.

Ch'en Ch'i-yüan, in his *Yung-hsien-chai pi-chi*, tells of the assistance given by Captain Roderick Dew, of the British navy, and by Le Brethon de Caligny, an officer of the French navy who trained and commanded a Chinese army, in the fighting against the Taip'ing rebels in Chekiang, especially at Ningpo and Shaoxing, and praises them highly. This paper presents a translation of this passage from the *Yung-hsien-chai pi-chi*, with further information on the activities of these two men from other Chinese and European sources.

DR. ALBERT P. LUDWIG: Chinese Traditional Theories of Foreign Relations.

DR. WOODBRIDGE BINGHAM; Wên Ta-Yü: The First Recorder of T'ang History.

In 617 A. D. at Tai-yüan in Shansi Li Yüan (T'ang Kao Tsu) took up arms against the Sui dynasty. With a large following, including his son Li Shih-mín (Tai Tsung), he seized Ch'ang-an and founded the T'ang dynasty (618-906). Later official histories accord chief credit to the son. But *The Court Journal of the Founding of the Great T'ang* depicts the father as the real leader and varies in other details. This *Court Journal* was written by a scholar attached to Li Yüan's headquarters, the Recorder Wên Ta-yü. Wên's brief biography in the *Old T'ang History* is here translated and his importance estimated.

MR. J. ARTHUR MACLEAN (Toledo Museum of Art): Some pertinent Facts about the Japanese Wood-Block Designer Hashiguchi Goyo.

Hashiguchi Goyo was a Japanese painter and wood-block print designer of great promise who died in 1921. During his later years, particularly the last two, he designed and had published thirteen titles, every one of which proclaims him the greatest exponent of wood-block designing since the famous artists of the Ukiyoe School. Unfortunately his thirteen titles constitute the sum total of his work in this field. The paper deals particularly with these thirteen titles, also the life of the artist. The titles in the order of publication are: (1) *Nude Woman after the Bath* (1915); (2) *Woman with a Mirror* (1918); (3) *Landscape—Yahskei* (1918); (4) *Landscape—Twilight at Kobe* (1920); (5) *Sanjo Bridge, Kyoto* (1920); (6) *Snowscape—Ibukiyama* (1920); (7) *Waitress with Tray* (1920); (8) *Woman applying Rouge* (1920); (9) *Woman Combing her Hair* (1920); (10) *Woman in Negligee* (1920); (11) *Woman in Summer Clothes* (1920); (12) *Woman after a Bath* (1920); (13) *Mandarin Ducks* (1920).

DR. HOMER H. DUNS (Library of Congress): The Fall of the Former Han Dynasty.

The fall of the Han dynasty in 8 A. D. was peculiar in that it was a

natural result of the Confucian theory of government, which had been whole-heartedly adopted by that dynasty. That theory gave the emperor no initiative, merely permitting him to select and dismiss the important officials and to ratify their decisions. He was sacrosanct, secluded. He consequently became a play-boy without the practical experience needed to select trusted officials and came to depend upon his maternal relatives. Once a clan so related held the power for a long time and heirs were lacking to the throne, usurpation was inevitable.

Professor H. G. CREEL (University of Chicago): On a New Method for the Teaching of Literary Chinese.

A project to create a corpus of materials for teaching non-Chinese students to read serious Chinese literature, and to use it as a tool for research, has been set up at the University of Chicago, with a staff of three scholars. It employs new etymological analyses, based on recent paleographic research, and is intended to produce a graded series of classical, historical, and literary texts, completely annotated. They will be accompanied by artificial exercises employing the vocabulary introduced by the text.

Rev. J. M. MENZIES: *Kings and Queens of the 'Shang Dynasty, as Revealed in the Oracle Bones of Honan* (illustrated).

The Society was tendered a luncheon by Western Reserve University in Hayden Hall of Flora Stone Mather College, at which over one hundred were in attendance. President Sturtevant expressed the Society's thanks to Western Reserve University for its hospitality, President Leutner of Western Reserve University responding in the name of his institution.

## FIFTH SESSION

The fifth session of the Society was called to order in the Assembly Room of the Cleveland Art Museum at 2.45 P. M. of the same day with President O. R. Sellers of the Middle West Branch presiding.

Director William M. Milliken welcomed the Society to Cleveland on behalf of the Cleveland Art Museum, indicated that the Museum would be kept open after the usual closing hour so that members might view the exhibits, and invited those in attendance to tea after the close of the session. President Sellers expressed the thanks of the Society to Director Milliken and the Cleveland Art Museum for their hospitality and the use of the Museum's facilities.

The following communications were presented:

Dr. NABIA ABBOTT (University of Chicago): Arabic Papyri of the Reign of al-Mutawakkil 'Ala Allah (A. H. 232-47/A. D. 847-61).

The historical background of these papyri is al-Mutawakkil's Act of Succession (A. H. 235/A. D. 850) dividing the empire among his sons Muhammad, al-Zubair and Ibrahim. Syria went to Ibrahim and Ibn al-Mudabbir was appointed as its financial governor. The documents themselves are returns of a tax land survey (A. H. 240/A. D. 855) ordered by al-Mutawakkil, issued by Ibn al-Mudabbir and effected by surveyors under the supervisor of building. The papyri are specially interesting, being the only ones known that deal with Syrian tax-surveys, and that reveal the basis on which tax rates were determined. They present peculiar problems for solution, for proof both of Ibn al-Mudabbir's efficient administration, and of the trustworthiness of later Muslim financial "Theorists."

Professor ALBERT H. LYNNER (University of Illinois): On the Trail of Mohammed the Conqueror.

Dr. JAMES MARSHALL PLUMER (University of Michigan): The Site of the Ware of Yüeh (illustrated).

Description of my visit in July, 1935, to kiln site which produced T'UNG ware or PI-SU-YAO, description of location, description of excavated material, together with suggestions as to importance of the ware for our knowledge of Chinese ceramics, firstly, as showing wide range in quality; secondly, as having had wide distribution; thirdly, as probable precursor of LUO-CH'UAN; fourthly, as distinguishable from "Northern CELADON"; fifthly, as prototype of Korean CELADON; sixthly, as possibly PER-T'ANG.

Professor W. F. ALDRIDGE (Johns Hopkins University): The Excavation of Bethel (illustrated).

After successful soundings under my direction in 1925, the excavation of Bethel north of Jerusalem was undertaken by a joint expedition of the American School of Oriental Research in Jerusalem and the Pittsburgh-Xenia Theological Seminary in 1934. The preparation of the material for publication is nearly completed and the chronology of the site is now clear in nearly every period of its history. This paper will stress the treatment of archaeological problems in this excavation.

Professor M. SPENGLING (University of Chicago): The New Pahlavi Inscription of Naksh-e-Rustem (illustrated).

A brief descriptive statement, illustrated by slides, on the new major Pahlavi inscription found by the Oriental Institute on the Ka'aba of Zoroaster at Naksh-e-Rustem last summer and given preliminary publication in *AJSL*, January 1937.

Bishop W. C. Wherry: Pictorial Art in Ancient China.—Tile Pictures of 3rd Century, *B. C.* (Illustrated).

The so-called Han pottery tomb tiles are usually stamped with stylized decorative designs. Of recent years similar tiles, but with a freer and more naturalistic style of decoration, have been found in Western Honan in considerable numbers. They mostly depict hunting scenes, and animals and birds, hunters and soldiers, are given in great variety and with a freshness and vitality that only a born artist could create. They appear to be the work of a single artist, or possibly a particular school of artists. In spirit these drawings differ so greatly from Han tiles that Chinese are calling them *Ch'in*. Evidence of script written on the tiles points to the third century *B. C.* (See *Illustrated London News*, October 24, 1930, pages 722-724).

Professor W. NORMAN BROWN (University of Pennsylvania): A Jaina Manuscript from Gujarat Illustrated in the Early Western Indian and Persian Styles (Illustrated).

A manuscript of the Jaina *Kulvasūtra*, obviously made in Gujarat in the 16th century, shows ornamentation and paintings illustrating the text, executed in both the Early Western Indian style and Persian, probably Timurid, style. The two styles are never mixed in the same painting, although examples of both styles may appear on the same page. This sort of unblended association of the styles gives place by at least as early as the last quarter of the 16th century to a blended style, which is the Gujarat, or Western Indian, variety of *Rajput*.

Miss DOMENICA BLAIR (Toledo Museum of Art): Influences of *Yō-yō* upon Japanese Design (Illustrated).

*Yō-yō*, the Japanese phase of the philosophy of opposites, has exerted a profound influence upon Japanese culture. It is discernible in architecture, in painting, in poetry, in costume, in conversation, and more or less in all phases of Japanese living. In design it imparts a rhythm expressive of fluidity and constant potential change. Vitality and vibrancy are the result. Through its use figures and patterns, and even blank spaces, become active and alive.

After the close of the session tea was poured for the members of the Society by wives of staff members of the Museum.

At 7.30 *P. M.* on the evening of the same day the Society held its Annual Subscription Dinner in the Grand Ball Room of the Hotel Statler. One hundred and twenty were in attendance. The Secretary presented the greetings of Professor Gustav Dalman and Professor and Mrs. A. V. Williams Jackson. In a post-prandial address, Mr. Mortimer Graves, Assistant Secretary of the American Council of Learned Societies, interpreted the meaning of the recent changes in Russia and the Far East as seen in the course of a recent visit to this part of the world.

## SIXTH SESSION

The sixth session was introduced by two business meetings, one of the Middle West Branch, the other of the Society as a whole.

## MIDDLE WEST BRANCH BUSINESS MEETING

The business meeting of the Middle West Branch, being its twenty-first annual session, was called to order by President O. R. Sellers at 9.00 A. M. on April 2 in a parlor of the Hotel Statler.

The Branch elected Professors L. Waterman, F. W. Buckler, and T. J. Meek as its Committee on Nominations for the year 1937-38.

In the absence of the Secretary-Treasurer the President reported for him as follows:

## OPERATING STATEMENT

1936		Receipts	Disbursements
May 13	Remittance from Treasurer American Oriental Society.....	\$100.00	
May 23	Payment to Ovid R. Sellers (deficit)...		\$ 3.42
	Petty Cash (phone—stamps).....		1.00
1937			
Jan. 20	Remittance from Treasurer American Oriental Society.....	100.00	
	TOTAL.....	\$200.00	\$ 4.42
Mar. 17	Balance on hand.....		195.58
		\$200.00	\$200.00

## BALANCE SHEET

Assets		Liabilities	
Cash on hand.....	\$195.58	Surplus	\$195.18
Credit-printing .....	3.60		
TOTAL.....	\$199.18		\$199.18

March 16, 1937

ALLEN D. ALBERT, JR.,  
Treasurer.

This is to certify that the above has been audited and found correct.

WALDO H. DUMBERSTEIN,  
GEORGE G. CAMERON,  
RAYMOND A. BOWMAN,  
Auditors.

The Branch agreed upon the last week in March as the approximate time for the Twenty-second Annual Meeting, the exact time and the place being left in the hands of the incoming Executive Committee.

The report of the Committee on Nominations was read as follows:

*President:* Professor SHELDON H. BLANK, of the Hebrew Union College, Cincinnati.

*Vice-President:* Dr. GEORGE G. CAMERON, of the Oriental Institute, University of Chicago.

*Executive Committee:* Professor W. M. McGOVERN, of the Northwestern University, Evanston, Ill., and Professor OWEN H. SELLESS, of the Presbyterian Theological Seminary, Chicago.

*Secretary-Treasurer:* Mr. ALBERT D. ALBERT, Jr., of the Seabury-Western Theological Seminary, Evanston, Ill.

The officers thus nominated were duly elected for 1937-38.

The Business Meeting of the Branch adjourned at 9.20 A. M.

#### BUSINESS MEETING OF THE SOCIETY

The business meeting of the Society as a whole was called to order by President Sturtevant at 9.45 A. M. of the same day in Parlor 4 of the Hotel Statler.

President Morgenstern presented the following minute composed by a committee consisting of Professor C. C. Torrey, A. V. W. Jackson, and himself on the death of the Society's former President, Professor Richard J. Gottheil:

RICHARD J. H. GOTTHEIL, Professor of Semitic Languages at Columbia University, member of the American Oriental Society for half a century, died in New York City on the 22nd of May, 1936, in his 74th year.

He was born in Manchester, England, on the 13th of October, 1862, and came to the United States with his parents in 1873. His father, Dr. Gustave Gottheil, a scholar of note, was for many years Rabbi of the Temple Emanuel in New York City.

Richard graduated at Columbia College in 1881, and thereafter pursued advanced studies in Semitic Languages and Jewish Literature in Europe, at the Universities of Berlin, Tübingen, and Leipzig. It was at Leipzig that he received his Doctor's degree, in 1886.

His chief interest at first was in the Syriac language and literature, and his most important publications were in this special field. He also published extensively in Arabic and Hebrew. His interests were wide, and many subjects outside the field of Semitics were included in the list of his learned works.

His first appointment at Columbia University was as Lecturer in Syriac for the year 1896-1897. In the latter year he was made Professor of Semitic Languages, and from 1892 the title included the Professorship of Rabbinical Literature. At the time of his death he was the oldest member of the Columbia Faculty.

He joined the Oriental Society at its meeting in October, 1888. The Society, then about one-third of its present size, numbered among its active members Professors Whitney, Bloomfield, Haupt, Moore, Toy, Langman, Peters, Harper, Jackson, Adler, and Isaac Hall, to mention no others. Professor Gottlieb continued to be a very active and influential member of the Society, constant in attendance at its meetings, and contributing often to its *Journal*.

He was a prominent figure in the Zionist movement, and was President of the American Federation of Zionists from 1898 to 1904. From 1898 onward he served as head of the Oriental Department of the New York Public Library. In the year 1899-1910 he held the post of Director in the American School of Oriental Research in Jerusalem; and in 1920-1921 he was Exchange Professor at the University of Strasbourg. A member of numerous learned societies at home and abroad, he was the recipient of honorary degrees and titles attesting his well-earned reputation.

Here in our Oriental Society his memory will be cherished especially by the many of its younger members who were his pupils, and by the old-timers who were so long and so pleasantly associated with him in the various branches of his work.

CHARLES C. TORREY, *Chairman*,  
A. V. WILLIAMS JACKSON,  
JULIAN MORGENSTERN.

The minute was accepted by a rising vote.

The Secretary presented the names of three persons now entering their fiftieth year of membership in the Society, namely, Professors James R. Jewett, Herbert W. Magoun, and Ira M. Price. Professor Price was present at the meeting. It was voted to send congratulatory telegrams to Professors Jewett and Magoun.

The Secretary read a telegram from Professor Charles R. Lanman answering the greetings extended to him by the Society in connection with the business session of March 31, and a telegram of greetings from Professor Georg Steindorff. It was voted to reciprocate Professor Steindorff's greetings.

Professor Kenneth S. Latourette presented the report of the Nominating Committee, on behalf of Professor Walter E. Clark, chairman of the Committee. The following were nominated as the officers of the Society.

*President:* Professor LEROY WATERMAN (University of Michigan).

*Vice-President:* Professor LEROY C. BARNET (Trinity College, Hartford).



*Secretary-Treasurer:* Professor CARL H. KRAELING (Yale University).

*Editor:* Professor W. NORMAN BROWN (University of Pennsylvania).

*Associate Editors:* Dr. JOHN K. SHRYOCK (Philadelphia).

Professor EPHRAIM A. SEITZ (University of Pennsylvania).

*Librarian:* Professor ANDREW KEOGH (Yale University).

It was voted to instruct the Secretary to cast a ballot electing these nominees to office. The Secretary cast the ballot and the officers were declared elected:

On behalf of the Nominating Committee Professor Latourette presented the following as nominees for Committee memberships:

*Executive Committee:* Dr. CHARLES OGDEN (New York), to serve for three years.

*Committee on Nominations:* Professor HENRY J. CADSWY (Harvard University), Professor CYRUS H. FRANK (Columbia University), Professor EDGAR H. STURTEVANT (Yale University), each to serve for two years.

It was voted to instruct the Secretary to cast a ballot electing these nominees to office. The Secretary cast the ballot and the nominees were declared elected.

On nomination of Professor Latourette from the floor it was voted to appoint Professor A. W. HUMMEL chairman of the Committee on Nominations.

On nomination of Professor F. Edgerton it was voted to appoint Professor N. SCHMIDT representative of the Society on the American Schools of Oriental Research.

President Sturtevant announced the appointment by him of the following Committees:

*Auditors:* Professors MILLAR BURNOWS, FERRIS J. STEPHENS.

*Committee on Collecting Archival Material bearing upon the History of the Society:* Professors F. EDGERTON (chairman), L. BARRET, L. BULL, C. J. OGDEN.

*Committee to consider the Disposal of the Society's Library:* Professors T. J. MEKE (chairman), H. H. BENDER, J. MORGENTHAU.

Dr. Charles J. Ogden presented the report of the Committee on Resolutions as follows:

The American Oriental Society desires now, as its 140th meeting draws toward the close, to express its appreciation and gratitude to its hosts on this occasion.

We extend our thanks to the President and Trustees of Western Reserve University for their hospitable entertainment, sincerely trusting that increasing prosperity and widening usefulness may be the lot of their institution, and likewise to the Cleveland Art Museum and the Cleveland Public Library for their kindness in giving us their facilities for our sessions and for the opportunity to view their treasures of Oriental art and literature.

We also warmly appreciate the personal attention and unfailing interest of President W. G. Lautner of Western Reserve University and the efficient planning of the details of the meeting by Professor Buckler and his associates on the Committee on Arrangements, as well as the courtesies extended to us by the Faculty, the Rowland, and the University Clubs. We are especially indebted to the Cleveland Oriental Club for entertaining at dinner the members of our Executive Committee. To all the representatives of Cleveland with whom we have been brought in contact we would say that they have cooperated to make our brief stay in their city one long to be remembered with pleasure.

It was voted to adopt these resolutions and to instruct the Secretary to send copies of them to the institutions which they concern.

The Secretary presented the action of the Executive Committee accepting the invitation of the University of Pennsylvania, the Dropsie College, and the Philadelphia Oriental Club to hold the next meeting at Philadelphia, at the occasion of the local Oriental Club's fiftieth anniversary.

The business session was declared terminated and the following communications were presented:

Dr. A. K. NIZAL: Ibn al-Djawzi's *Dhamm al-hawâ*.

Some details concerning this most interesting book were published by H. Ritter in *Der Islam* XXI (1933), p. 87. The present writer has prepared a critical edition on the basis of the five manuscripts cited by Ritter; the Vatican manuscript, since discovered by Levi Della Vida, though of minor importance, has also been taken into consideration. The book contains a wealth of rare anecdotes and verses, some of them hitherto unknown.

Mr. ROBERT M. ERDMAN (Oriental Institute): *Archaeology and Hyksos Dates*.

The Hyksos period may be divided archaeologically into two main stages. The first appears to have been well formulated by the time of Sesostris II (ca. 1900) judged by Egyptian and Syrian evidence. A process of infiltration preceded establishment of the Hyksos dynasty. The second period, characterized largely by a 'Hurrian' element, may be dated ca. 1600 in Egypt. It was the people of this period (the XVIIth dynasty) who were expelled by Ahmose. The Hyksos as an integrated cultural group were not extinguished on Asiatic soil until defeated by Thutmose III and Amenhotep II.

Professor WILLIAM M. MCGOVERY (Northwestern University): *The Ephthalites.*

Among the early inhabitants of Central Asia the Ephthalites are unique in being mentioned in a wide variety of sources, Chinese, Indian, Persian, Armenian, and Greek, and yet they are still shrouded in mystery. Were they Huns, or "White Huns" as suggested by the Classical authors? The Chinese sources show they were not, though subject to Hunnish influence. What language did they speak? Some scholars have suggested Turkish, others Tibetan, others Mongolian. From the Chinese sources it seems probable that they originally spoke an Indo-European language. Their connection with the Juan-Juan or "Avars," and the date of their migration from Zongaria to Bactria.

Professor SAMUEL H. BLANK (Hebrew Union College): *The Death of Zechariah in Rabbinic Tradition.*

Later tradition thoroughly confused the three Zechariahs of the Bible: (1) Zechariah the son of Jehoiada, the priest (2 Chron. 24: 17-22), (2) Zechariah the son of Jeherechyah, Isaiah's witness (Isa. 8: 1 f.), and (3) Zechariah the son of Iddo, the prophet. A well-known example of this confusion occurs in Matt. 23: 34 f., where the present text suggests that Zechariah the prophet suffered death by violence in the Temple. Actually this and similar passages refer to the son of Jehoiada whom the people stoned. About his death an interesting tradition evolved which this paper seeks to trace to its source as an illustration of the growth of a legend.

Professor W. A. IRWIN (Oriental Institute): *The Immanuel Oracle.*

The form of the legend apparent in the Immanuel Oracle and parallel Old Testament passages reveals that Isa. 7: 14 has been traditionally mistranslated. The same approach would indicate that vs. 16 is spurious, but vs. 17 in the main genuine. Isaiah is here threatening Ahaz for his incredulity, that he and the entire Davidic dynasty will be deposed by the wonder child who in the actual course of events was presently born to himself and the "Almah."

Rev. MICHAEL J. GRUNTHAMER, S. J. (St. Mary's College): *The Future Life in the Psalms.*

There are not a few indications in the Psalms that their authors believed in the survival of the soul after death. This conclusion is not invalidated by certain expressions which seem to imply the negative. The problem is whether at least some of them entertained a certain hope of happiness for the righteous in the future life. A careful scrutiny of Ps. 10, 49, 73 seems to show that this was really the case. In fact even a resurrection of the body appears to be clearly taught.

Mr. JOSEPH L. MICHAEL: *A Reconstruction of the Story of the Fall of Jerusalem as Given in the Book of Jeremiah.*

The paper presents a critical analysis of chapters 39, 40, and 41 of

the Book of Jeremiah, in the effort to determine the authenticity of the various accounts which deal with the fate of Jeremiah, and to reconstruct the sequence of the events that took place at the time and after the capture of Jerusalem by the Babylonian army in 586 B. C.

The meeting adjourned at 1 P. M.

Members whose special field of interest is the Far East held a luncheon meeting at the Hotel Statler between the sixth and seventh sessions.

### SEVENTH SESSION

The seventh and last session of the meeting was called to order in Parlor 4 of the Hotel Statler by President Sturtevant at 2.10 P. M. of the same day. The following papers were presented and discussed:

MR. CHARLES F. KRAFT: A Critique of some recent Theories of Strophic Structure in Hebrew Poetry.

A rapid survey of the history of the investigation of the subject leads one to question the validity of current indifference or skepticism concerning the identification of strophic structure in Hebrew poetry and to examine divergent conclusions reached to date. Critical summarization of the theories of the Müller-Zimmer-Möller school, of Condamin, of Desnoyers, and of some American scholars is undertaken. An evaluation of recent research on the subject points to some guiding conclusions for further study.

Professor W. F. STUBBS (Duke University): Hadrian in Palestine.

Among certain obscure spots in the record of Hadrian's reign are the details of his Syrian journeys and the date and motivation of his decision to rebuild Jerusalem as a Roman city. An inscription recently found at Jerash, Transjordan, throws some light on these matters.

Professor F. W. BUCKLER (Oberlin Graduate School of Theology): The Need for an Historical Atlas of the Near and Middle East.

Professor HERBERT G. MAY (Oberlin Graduate School of Theology): Solar Aspects of the Jerusalem Cult.

Ezekiel's description of the temple rites (ch. 8) and his vision of the departure of Yahweh's Glory (ch. 10) are to be interpreted in the light of ritual following the summer solstice, marking the descent of the sun-god from his throne in the heights of the heavens. Psalm 22, Isaiah 14, and the Ugarit tablets further illuminate these rites. The return of Yahweh's Glory occurs at the spring equinox. This study illustrates the cultural approach as a check on literary criticism. The throne-chariot vision is not a later addition to the book of Ezekiel.

Professor KEMPER FULLERTON (Oberlin Graduate School of Theology): Remarks on Isaiah's Attitude during the Sennacherib Campaign of 701.

In AJSL of Oct. 1925 the attempt was made to determine Isaiah's political policies from the time of Tiglath-pileser (735-732) through the campaigns of Salmuneser (721), Sargon (720 and 713-711), to the reign of Sennacherib, 705 B. This period covered the rise and development of an anti-Assyrian party in Judah, the pro-Assyrian party under Ahaz giving place more and more to the anti-Assyrian party after the death of Ahaz and the accession of Hezekiah ca. 720. This party consistently opposed by Isaiah but in vain. It gained supreme control and helped to precipitate the catastrophe of 701. Logically, Isaiah should have remained anti-Assyrian. Did he do so? This leads to examination of the very distinct block of prophecies in chs. 29-31. Reasons for dating these prophecies in 701. This block of prophecies very peculiar in arrangement. Doom regularly followed and cancelled by hope. 29: 1-4 cancelled by v. 5-6; 29: 7-22 by 23-29; 30: 1-4 by 5-8; 30: 9-15 by 16-24; 30: 1-17 by 18-26 and 27-33; 31: 1-4 by 5-9. Reasons for eliminating the hope passages. When eliminated Isaiah is consistent throughout his life in his opposition to the anti-Assyrian party.

Mr. P. E. SOMMER (Cleveland Public Library): Coordination and Standardization of Oriental Transliterations.

Lack of coordination through too much specialization. Mohammedan names. Hindu names. Purpose of scientific transliteration: reproduction of spelling, not pronunciation. Complications which arise from attempts to bring out at the same time the pronunciation. Lack of uniformity even within the same field—International standardization. Familiarizing the public at large with some international sound symbols.

Professor ROBERT H. HALL (University of Michigan): Tokaido: Road and Region (Illustrated).

From remote times down to the present, the concepts of road and region have been much the same to the Japanese people. The great geographical divisions of Japan have been recognized from earliest times and each one has been identified with the road which served it and tied it to the seat of government. Tokaido was early brought under Japanese control but until the foundations of Kamakura it was a region of secondary importance. The main trunk line railroad of present-day Japan follows closely the trajectory of the ancient roads of Tokaido.

Professor J. J. L. DUYENDAEK (University of Leiden): The Particle *i* in Written Chinese.

The following papers were presented by title:

Dr. E. J. JOURJ (Institute for Advanced Study, Princeton): The *Ishtraqi* Revival of al-Suhrawardi.

Dr. LEONOR L. UHL: Leading Buddhist Settlements, in Telugu Land, South India, along the Kistna River, for a Thousand Years.

Professor ROSWELL S. BRITTON (New York University): The Yin Hsü Ossigraphy.

Mr. HARDIN T. MCCLELLAND: (a) Critical Historicism and Classical Research in Modern Chinese Philosophy; (b) Vitalism and Humanism in Modern Chinese Philosophy.

Professor HENRY S. GRIFFMAN (Princeton University): The Scheide Papyrus of Ezekiel and Its Relation to the Massoretic Text.

Dr. DAVID I. MACHT: Physiological Appreciation of Biblical References to Bone and Marrow.

Mr. CHARLES F. KNAPP: The Religion of Israel as reflected in the Book of Amos.

Professor FRED V. WIDNETT (University of Toronto): The Relation of the Mandaeen Script to the other Aramaic Scripts.

Mr. KURI F. LEIDIGER: The Pictorial Philosophy of India.

Professor FRANK R. BLAKE (Johns Hopkins University): The Representation of the Original Semitic Short Vowels in Hebrew.

Mr. MURRAY B. EMENEAU (Yale University): A Reflexive-reciprocal formation in the Coorg Language.

Dr. E. R. LACHEMAN: (a) The Hurrian Elements in Nuzi Documents; (b) The Babylonian Collection of Spencerian College in Cleveland.

Dr. NANCY LEE SWANN (Gust Oriental Library): Chao I-ch'ing's Study of the "Water Classic."

Dr. H. HENRY SPOHN (City Hospital, Welfare Island, N. Y.): Arabic Magic Bowls II. An Astrological Bowl.

Professor ALLEN D. ALBERT (Schenbury-Western Theological Seminary): Canals and Flood Control in Ancient Mesopotamia.

The meeting adjourned at 4.45 P. M.

## THE COMPLAINT OF LABAN'S DAUGHTERS<sup>1</sup>

MILLAR BURROWS

YALE UNIVERSITY

WHEN JACOB made up his mind to leave the house of his father-in-law and return to the land of Canaan, he called his wives out into the field and informed them of his intention. They expressed their approval and their willingness to go with him in these words: "Have we any longer a share or an inheritance in our father's house? Have we not been reckoned by him as foreign women? As a matter of fact (וְכֵן), he has sold us; indeed he has eaten up our money. All the wealth that God has taken away from our father belongs to us and our children. Do what God has told you to do" (Genesis 31:14-16).

Commentators have often remarked that this statement gives us a valuable glimpse into the social ideals and customs of Old Testament times. That the picture presented reflects historical conditions

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<sup>1</sup> The following abbreviations will be used in this article: AASOR—*Annual of the American Schools of Oriental Research*; BASOR—*Bulletin of the American Schools of Oriental Research*; BCB—Berry, "The Changing East" (*Colgate-Rochester Divinity School Bulletin*, March 1935, pp. 3-36); GEN—Chiera, *Excavations at Nuzi* (*Publications of the Baghdad School*); CTC—Contenu, *Textes Cuneiformes* (Louvre); DMAL—Driver and Miles, *Assyrian Laws*; DMI—Dussaud, "Le 'mohar' israélite" (*Comptes rendus, Acad. des Ins. et Belles Lettres*, 1935, pp. 141-51); DVW—David, *Form en Wesen van de Huwelijksverhouding naar de Oud-Oosterse Rechtsopvoeding* (Leiden 1934); GMC—Grandquist, *Marriage Conditions in a Palestinian Village*; GNT—Gordon, "Nuzi Tablets Relating to Women" (*Miscellaneous Orientalia* 163-88); GPN—Gordon, "Parallèles Nouziens aux Lois et Coutumes de l'Ancien Testament" (*Revue Biblique* 1935, 1-8); GSW—Gordon, "Status of Women" (*Zeitschrift für Assyriologie* 1936, 147-69); GTK—Gadd, "Tablets from Kirkuk" (*Revue d'Assyriologie* 1929, 49-101); HSS—*Harvard Semitic Series*; KF—Koschaker, "Fratriarchat" etc. (*Zeitschrift für Assyriologie* 1933, 1-89); KNR—Koschaker, "Neue Keilschriftliche Rechtsurkunden" (*Abh. d. Sachs. Gesellsch. d. Wissensch.* XXXIX, 5); Koschaker, KQU—"Quellenkritische Untersuchungen" (*Mittell. d. Vorderas. Ges.* 1921); KRS—Koschaker, *Rechtsvergleichende Studien*; KUH—Köhler u. Ungnad, *Hammurabis Gesetz*; NBG—Neuhäuser, "Beiträge zur Gesch. des bib.-altjud. Eheschlussungsrechts" (*Mittell. d. Vorderas. Ges.* 1920); SBR—San Nicolò, *Beiträge zur Rechtspraak.*; SKM—Smith, *Kinship and Marriage*; SUA—Schorr, *Urkunden des Altbabyl. Zivil u. Prozessrechts*.

we have no reason to doubt. For what period of history it is true, however, is another question. We must reckon with the possibility of what I may call retrojection,<sup>2</sup> i. e., unconscious anachronism, naïvely attributing to the patriarchal period what was true of the writer's own day. Thus some writers have regarded our passage as showing that in the eighth century, when the E-document is supposed to have been written,<sup>3</sup> the Israelites no longer recognized a father's right to the use of his daughter's bridal money.<sup>4</sup> On the principle that ancient narratives in general are primary sources for the times in which they were written rather than the times of which they purport to tell, this is the only sound position to take, unless we have other evidence of the conditions portrayed for the period to which the story refers.

Such evidence is not wanting. Recent Old Testament scholarship exhibits a notable tendency to regard the stories of the patriarchs, and particularly the Jacob stories, as portraying faithfully the conditions of the pre-Mosaic period.<sup>5</sup> Especially striking parallels have been found between these stories and the mixed culture of northern Mesopotamia in the Amarna period, as revealed by the now well known Nuzi documents.<sup>6</sup> Now it happens that two or three of these points of contact are found in the very passage we are considering. An examination of them will help us to estimate the historical significance of the passage.<sup>7</sup>

<sup>2</sup> Historians sometimes say that ancient writers have "projected" the customs and ideas of their own times into the earlier periods of which they tell. Since it is anomalous to speak of projecting backwards, I prefer to speak of "retrojection," and the term is a convenient one for this very familiar phenomenon.

<sup>3</sup> The arguments of Rudolf and Volz have shown that it is very doubtful whether there ever was a separate E-document, but that point does not concern us here, nor does the date of composition of what in any case may be called E-material.

<sup>4</sup> So Benzinger, *Exeget. Bib.* iii, col. 2043; cf. also the commentaries in loc. and DVW, note 100.

<sup>5</sup> Cf., e.g., Albright, *The Archaeology of Palestine and the Bible*, 120-51; Graham and May, *Culture and Conscience*, §1-55.

<sup>6</sup> For these parallels see particularly GPN, and with special reference to the Jacob story BASOR 60.25-7.

<sup>7</sup> Since in this paper I have had to take positions on matters belonging to a field in which I can claim no competence, I submit by way of extenuation that I have endeavored to avoid error by consulting experts in that field. I wish to express here my indebtedness for such help to my colleagues



In the first place we may remark that Jacob's marriages conform closely in form to the *errēbu*-marriage of ancient Babylonia.<sup>8</sup> Ordinarily the Babylonian family was thought of as continued by the male descendants. Wife and children therefore belonged to the husband's family. When there was no son, however, the family might be continued through a daughter by taking a husband for her into her father's family as an *errēbu*, comparable to the "visiting husband" among Palestinian peasants today.<sup>9</sup> A similar arrangement appears in the Nuzi tablets.<sup>10</sup> Two instances have been noted<sup>11</sup> in which a man who has no son adopts a young man as his heir, at the same time giving him his daughter in marriage.<sup>12</sup>

Several scholars see this type of marriage also in the Assyrian Code,<sup>13</sup> which refers a number of times to wives living in their fathers' houses.<sup>14</sup> Driver and Miles argue that these references do not necessarily imply a distinct form of marriage in which the wife remains in her father's house; what is meant may be simply that the woman has left her husband and returned to her own family, or that owing to special circumstances she has never left her child-

Professors Albrecht E. Goetze and Ferris J. Stephens. Of course they are not responsible for any errors I may have committed in spite of their assistance.

<sup>8</sup> For a description of this type of marriage and references to the sources cf. DVW 4 f, 18 f, and notes 110-113. David notes the connection between Jacob's marriages and *errēbu*-marriage, as does also Gordon, BASOR 66, 26. A translation of the Sumerian Laws (one *ittišu* series), containing the prescriptions with regard to *errēbu*-marriage, is given by Gressmann, *Altoriental. Texte zum A. T.*, 2nd ed., pp. 410 f. A commentary by Landsberger has just been published by the Pontifical Biblical Institute in Rome.

<sup>9</sup> Cf. GMC ii. 312 ff, where a case is cited in which a man came as herdsman to a girl's father, served for the daughter, and remained in the father's household, just as Jacob did.

<sup>10</sup> DVW, note 121.

<sup>11</sup> GTK, no. 51; HSS v, no. 47 (AASOR x, no. 2). The former of these two tablets is the one referring to the heir's possession of the household gods (see now Gordon, BASOR 66, 25 f). The other tablet also exhibits several parallels with the stories of the patriarchs.

<sup>12</sup> The second of these tablets does not state that the bride is the daughter of the man who adopts the bridegroom, though David assumes this (loc. cit.), and Speiser regards it as probable (AASOR x, p. 22).

<sup>13</sup> E. g. Keschaker (KQU 60 f; KF 85-7), San Nicolò (SBR 21-3), and David (DVW 19 and note 113). See also references cited DMAL 136 ff.

<sup>14</sup> §§ 25, 26, 27, 32, 33, 36, 38.

hood home.<sup>15</sup> That is possible, but unlikely. The law states (§ 38) that if a wife is divorced while living in her father's house, her *terḫatu* or "bride-price"<sup>16</sup> belongs to her and cannot be claimed by her husband. If this refers to particular circumstances rather than a distinct type of marriage, it implies that under other circumstances, i. e. if the wife were living with her husband, he might demand the return of the *terḫatu*.<sup>17</sup> Flight to the paternal home would then be the recourse of any woman who feared that her husband might divorce her and reclaim her *terḫatu*. On the other hand, if the reference is to marriages of the *errēšu*-type, the law that the *terḫatu* belongs to the wife in case of divorce is equivalent to the provision of the Sumerian Laws that an *errēšu*-husband forfeits the *terḫatu* if he leaves his father-in-law's house.<sup>18</sup> San Nicolò maintains that this type of marriage was not only known in Assyria but was the characteristic form of Assyrian marriage in early times, and that the later Assyrian Laws betray an effort to subordinate it. The Hittite Code also (§ 37), as San Nicolò remarks, attests this form of marriage along with the more familiar type.<sup>19</sup>

In view of all this material, so near at hand, it is no longer necessary to bring in from far-off Ceylon the analogy of "beena" (*bina*) marriage.<sup>20</sup> The *mof'a* marriage of the Arabs<sup>21</sup> is nearer home and may go back to very early origins. It is important to observe, how-

<sup>15</sup> DMAL 130-42. Cf. *Mishna Pes.* 8.1 for special provisions regarding a married woman living in her father's house. The Nuzi tablets record cases of wives who have fled to their father's houses, and whose husbands have taken court action to compel them to return (ASOR x, nos. 34 and 38; cf. OSW 161).

<sup>16</sup> I adopt this translation provisionally, pending discussion elsewhere of the nature of the *terḫatu* and the Hebrew *mohar*. For convenience I also use throughout this paper the form *terḫatu*, disregarding actual variations of orthography in the sources.

<sup>17</sup> The preceding law (§ 37) says that a man in divorcing his wife "may give her something" if he so chooses; otherwise "empty she shall go out." This may be taken to imply that the *terḫatu* must be returned to the husband, though not necessarily, especially if the *terḫatu* has been retained by the wife's father.

<sup>18</sup> V. s., note 8.

<sup>19</sup> SBR 91-3.

<sup>20</sup> Cf. especially J. Morgenstern, "Beena Marriage in Ancient Israel and its Historical Implications" (*Zeitschr. für die alttest. Wissensch.* 1929. 91-110; 1931. 46-8).

<sup>21</sup> SKM 78 ff.

ever, that the relation of Jacob to Laban's family has nothing whatever to do with matriarchy. It belongs rather in the framework of *erêbêr*-marriage, which is essentially a product of patriarchal society. The purpose of such a marriage is to provide male succession for a man who has no son, like Sheshan in the Old Testament, who obtained an heir through his daughter by marrying her to his slave.<sup>22</sup>

The case of Jacob, it is true, is complicated by the fact that in one verse (Gen. 31:1) "the sons of Laban" are referred to. Gordon offers a simple explanation: Jacob had been with Laban twenty years, and the sons had been born in the meantime, since his coming.<sup>23</sup> In that case the question of Leah and Rachel, "Have we still any portion or inheritance in our father's house?" (verse 14), means that before their brothers were born they could have expected an inheritance,<sup>24</sup> but now the sons will inherit the estate. The daughters' claim that everything which Jacob has won from Laban belongs to them and their children (verse 16) will then mean, "Since you have acquired all this, it now belongs to us and our children instead of our brothers, so that it is to our interest to go with you." Commentators, however, remark that verse 1 is by common consent attributed to J, and in the E-strand of the story there is no indication that Laban had any heir apart from his daughters.<sup>25</sup> If the sons may be left out of account as not belonging to the original story, the reason Rachel and Leah no longer expect an inheritance from their father will be that Jacob's gains at Laban's expense have left no estate for them to inherit. On that basis the statement that what Jacob has won belongs to them and

<sup>22</sup> 1 Chron. 2: 34 f. David calls attention to Barn 2: 61 (Neh. 7: 63), in which he sees an indication that in such marriages the husband took the name of his father-in-law (DWW, note 119). He also remarks that Ring (*Israel's Rechtsleben* 16) and Lods (*Jeruz* 213) find this form of marriage in other biblical passages. Grandquist, in connection with the modern Palestinian custom (v. s.), refers to Samson's marriage, Judges 14: 18 (GMC II, 313).

<sup>23</sup> BASOR 66, 26.

<sup>24</sup> David argues that what is implied is that Jacob was Laban's heir, because daughters could not inherit in Israel before the time of Numbers 27 (DWW, note 119). Gordon infers from Nuzian parallels that Laban had actually adopted Jacob (BASOR 66, 25). Procksch (in loc.) remarks, however, that in the ancient Aramaean *Kulturbereich* women may already have had such an independent position as is here presupposed. At Nuzi a daughter might inherit property when there was no son (GSW 159).

<sup>25</sup> Cf., e.g., Skinner and Procksch in loc.

their children will mean, "Since what you have gained would have fallen to us anyway, we are justified in carrying it off with you." Jacob's recital of grievances (vs. 36-42) does not include any reference to Laban's sons as coming between him and the inheritance of the estate. But by either view of the reference to the sons the supposition that Jacob had entered Laban's family as an *errēbu* provides an explanation for the stealth with which he departed, taking wives and children and flocks; <sup>22</sup> it explains also the indignant protest of Laban when he overtook them (verse 43): "The girls are my daughters, and the children are my children, and the flocks are my flocks, and everything you see is mine." <sup>23</sup>

Laban's daughters complain that their father has treated them as "strangers" or "foreigners." One is reminded of the unenviable position of the "strange wife" among the *fellāḥin* of Palestine today. A woman from a distant village is at a disadvantage in married life, because her father and brothers are not at hand to see that she receives fair treatment from her husband. <sup>24</sup> Laban, instead of being a protector to his daughters, has acted toward them as though they were outsiders; they can therefore count on nothing from him that would induce them to remain at home.

There are echoes of a similar conception in the cuneiform literature. An Assyrian document, for example, specifies that a certain man who is adopting a girl shall treat her "as his own daughter, as an Assyrian." <sup>25</sup> In like manner a Nuzian tablet of "daughter-ship and daughter-in-law-ship" states, "And Hanate shall treat Halpappuša as a daughter of Arrapha; she shall not return her to (the status of) a slave-girl." <sup>26</sup> The implication of this and similar expressions is that a woman living among her own people receives and has a right to expect better treatment than one from another land and people, <sup>27</sup> just as the words of Leah and Rachel imply that

<sup>22</sup> According to the Sumerian Laws (v. s., note 3), lines 40-46, an *errēbu* who left the house of his father-in-law forfeited the *terḡess* he had paid (BWW 19); of course he could not take his wife with him.

<sup>23</sup> Gordon (v. s., note 24) takes this to mean that Laban had adopted Jacob, as in the Nuzi tablets referred to above (note 11), and therefore had patriarchal authority over him and his family. The inferior position of the *errēbu*-husband in the family provides a better explanation, though the situations assumed by both explanations have much in common.

<sup>24</sup> GNC I. 94-6.

<sup>25</sup> Cited without reference BMAL 166.

<sup>26</sup> AASOR xvi, no. 42, line 21.

<sup>27</sup> See further OSW 149 on the inferior status of foreign women at Nuzi.

the treatment they have received is such as normally only foreign women would receive.

Laban's unnatural attitude is demonstrated by the fact that he has "sold" his daughters, they claim. Neubauer<sup>32</sup> is quite right in saying that this passage does not prove the existence of marriage by purchase among the Israelites, since there would have been no point in complaining if it had been customary for fathers to sell their daughters.<sup>33</sup> Perhaps there is some force also in the remark of Driver and Miles that we must not take too seriously a mere "angry outburst on the part of two indignant women,"<sup>34</sup> not to mention the fact that this is no stenographic report of an actual conversation. One may even say that *mākar* does not necessarily mean "sell." D. Winton Thomas has recently argued that the Hithpael of this verb in three of its four occurrences in the Old Testament and its single occurrence in Ben Sira means "act deceitfully," like the second stem of the cognate Arabic verb.<sup>35</sup> On this basis one might take the Qal here (or, repointing, the Piel) to mean simply, "He has tricked us." The context, however, favors the usual translation. The most natural interpretation is that Laban had done something which made the marriage of his daughters in effect a mere sale of property.

What it was that he had done is stated in the next clause: he had "eaten" their "money" (*ʾakal kaspeni*). In cuneiform documents the *terḫatu* is often called simply the "money" (*kaspu*) of the bride. Many examples of this have been noted in the Nuzi tablets,<sup>36</sup>

<sup>32</sup> NBG 73, 206.

<sup>33</sup> As David remarks, however, Neubauer goes too far in maintaining that the passage disproves the practice of marriage by purchase (DVW, note 100).

<sup>34</sup> DMAL 142 n, 158 n.

<sup>35</sup> *Journal of Theological Studies* 1936, pp. 388 f.

<sup>36</sup> Most of the following references are cited by KE 17 n and GSW 167 n, but inasmuch as neither cites all of them or indicates both original publication and publication in transliteration, I repeat the list here: (1) GIK, no. 12, line 4 (v. l., note 80); (2) *ib.*, no. 35, line 20; (3) HSS v, no. 17 (AASOR x, no. 30), line 20; (4) *ib.*, no. 11 (AASOR x, no. 31), lines 6, 14, 27, 29; (5) *ib.*, no. 16 (GNT, no. XVIII), line 11; (6) *ib.*, no. 25 (AASOR x, no. 29), line 7; (7) *ib.*, no. 80 (AASOR x, no. 26), lines 12, 30; (8) HSS ix, no. 145 (GNT, no. XXXIV), line 11; (9) CTC ix, no. 7 (KNR, no. 24), lines 5-12; (10) CEN i, no. 78 (KNR, no. 261), line 8. To these may now be added (11) AASOR xvi, no. 30, line 16; (12) *ib.*, no. 54, line 6.

as well as in other cuneiform sources.<sup>37</sup> In simple marriage contracts the girl's "money" is given by the bridegroom to her father or guardian.<sup>38</sup> When a girl is adopted by a person who thereby acquires the right to give her in marriage, her "money" is paid by her husband to the adoptive parent.<sup>39</sup> Only once is the term applied to a sum of money given to the bride herself.<sup>40</sup> Where the amount of the "money" is stated, whether in simple marriages or in adoptions, it is regularly forty shekels.<sup>41</sup> Presumably, as has often been supposed, this "bride-price" is the "money" of Lahar's daughters.<sup>42</sup> Jacob, of course, had paid for his wives by labor instead of money; "we must suppose therefore that they considered themselves entitled to the equivalent of his labor in money, or at least to a part of it. Among the Palestinian *fellāḥin* today the *moḥr* may be paid in money, goods, or service.<sup>43</sup> At Nuzi also the *terḥatu* was not always paid in silver, nor does the use of the word *kaspu* necessarily imply this.<sup>44</sup>

Not only the noun is used in cuneiform documents in this way; the same verb also which we have in our Hebrew text appears sometimes together with this noun in the Nuzi tablets in connection with a father's or guardian's right to the bride's *terḥatu*.<sup>45</sup> Its meaning

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Since *kaspu* in its quasi-technical sense occurs four times in no. 4 of this list and twice in no. 7, we have here actually 16 instances. No. 5, it must be admitted, may not be a marriage transaction.

<sup>37</sup> E. g., in an Old Babylonian tablet the bride is said to have received (X) *šēḫil kaspiu kaspu* (te) *ir-ḫa-za* (SUA, no. 2, Schorr's transliteration).

<sup>38</sup> So note 36, no. 4, line 6, and nos. 6 and 12. In no. 2 and no. 4, lines 27 f., the *kaspu* is paid to the girl's guardian, but the person who pays it is not specified.

<sup>39</sup> Note 36, no. 4, lines 12 f., and no. 8. In no. 3, no. 7 line 30, and nos. 9 and 10 the adoptive parent receives the *kaspu*, but it is not explicitly stated that the husband pays it.

<sup>40</sup> No. 7, line 12. See below, p. 275.

<sup>41</sup> Nos. 6, 8, 12.

<sup>42</sup> The use of *keḫ* in Exodus 21: 35 may be noted in this connection.

<sup>43</sup> Neubauer's effort to show that Jacob's labor was not the equivalent of a *moḥr* (NBG 71 f.) cannot be regarded as successful. David considers it a *moḥr* (DVW 10). See also below, note 45.

<sup>44</sup> GMC i, 9 n, 119.

<sup>45</sup> GEW 156, GPN 3. Gordon remarks that in GEN iii, no. 297, line 26, *kaspu* is used of flocks. Cp. HSS v, no. 10 (GNT no. XVIII), line 11. See also Speiser, JACS 52 (1932), 335 and note, for the various uses of *kaspu* in *dittanatu* contracts.

<sup>46</sup> Nos. 2, 4, and 9 of note 36, above.

in such cases has been taken to be "have the usufruct" of the sum in question. Thus Gordon maintains that the purpose of the *kaspu-terhatu* was to provide for the maintenance of the wife in case, being divorced or widowed, she returned to her father's house. Meanwhile (which ordinarily meant as long as he lived) he enjoyed the usufruct of it. This, Gordon thinks, is the meaning of *akālu kaspa* in the Nuzi documents.<sup>47</sup> In the case of Jacob Gordon suggests that the *kaspa* of Laban's daughters consisted of flocks, and that Laban had appropriated the milk, wool, and natural increase.<sup>48</sup>

But the words of Leah and Rachel imply more than that their father had used the income from the "money." The emphatic form of their statement (perfect tense with infinitive absolute) suggests that he had completely consumed the "money" itself. This accords both with the primary meaning of the verb and with its secondary applications to the devouring action of fire, sword, pestilence, and the like. As a matter of fact, it seems to me, this is also its real significance in the expression we have noted in the Nuzi tablets. In Old Assyrian (Cappadocian) documents the verb *akālu* is applied to the use of income from a sum of money, but in such cases the object of the verb is a noun referring to the income itself, not to the capital. For example, a letter published by Sidney Smith<sup>49</sup> states that the writer is going to buy and send two talents of lead, and adds, "consume the profit from it."<sup>50</sup> In a legal document,<sup>51</sup> dealing with a suit arising from a *maruqqu* contract between a capitalist and the traveling merchant who acted as his agent,<sup>52</sup> the decision is made to depend upon whether or not the agent and his representatives have "consumed" the "third" to which he was

<sup>47</sup> So GEN 3, where the parallel with our passage is pointed out.

<sup>48</sup> *Ibid.*

<sup>49</sup> *Oncif. Texts from the Cappad. Tablets in the Brit. Mus.*, Part III (1925), no. 6. A transliteration and translation of this letter is given by Moshe Bar-Am, *Studies of Business Letters from Cappadocian Cuneiform Texts* (Yale Dissertation, 1935, unpublished). The letter is one of the documents referred to by Eissner and Lewy in discussing the meaning of the verb *akālu* (v. i., note 54).

<sup>50</sup> (19) *ni-ma-al-šu* (20) *a-kuhūl*.

<sup>51</sup> Sayce, *Babyloniaca* iv, pp. 72 ff; cf. also Landsberger, *Der Alte Orient* xxiv, 4; Eissner and Lewy, *MVAG* xxxv, no. 328. I am indebted to Prof. Lewy for calling my attention to this material.

<sup>52</sup> For a full discussion of this type of business relationship and of this tablet cf. Eissner and Lewy, *op. cit.*, II (*MVAG* xxxvi), pp. 86 ff.



entitled as his commission.<sup>53</sup> Other instances of the use of the verb in Cappadocian tablets for the consumption of sums of money are given by Eisser and Lewy.<sup>54</sup> In all these cases it seems clear that *akdu* does not mean "have the usufruct of" but "consume."<sup>55</sup> It may not be amiss to remark that Lewy describes the writers of these Cappadocian texts as Assyrianized West Semites, and that some of their personal names contain the element *Laban*.<sup>56</sup> Before leaving this subject an interesting parallel from present-day Palestine may be noted. Miss Grandquist tells of a widow who took pains when she was married again to see that her bride-price came into her own possession, because, as she said, her father had "eaten" the bride-price of her first marriage.<sup>57</sup>

The simplest interpretation of the complaint of Laban's daughters in the light of this material would be that Laban had used up their "bride-price," whereas (by implication) he should have either given it to them or held it for them in trust, using at most the income derived from it. Gordon concedes the possibility of such an interpretation. Admitting that the passages in which the expression *akdu kaspu* occurs are too few and inconclusive to substantiate fully his own view, he suggests that the *terhatu*, while originally intended for the wife's support in case of divorce or widowhood, may have tended to degenerate into a marriage-price retained by the guardian.<sup>58</sup> Laban's conduct would then be an example of this tendency. By appropriating for himself what he should have held as a trust-fund he made his daughters' *kaspu* a mere purchase-price. Whether the *terhatu* had always been intended as such a trust-fund or whether this view of it was a relatively late development, our passage implies that Laban should not have "consumed" the "money."

<sup>53</sup> [10] *ka-li-is-tam* (20) *a-ku-lu-ma*; (25) *ka-li-is-tam* (26) *la a-ku-lu*.

<sup>54</sup> Op. cit., II, 96 n.

<sup>55</sup> According to Bezold, *Babyl.-Assyr. Glossar*, *akdu* is sometimes used of a field, where of course it can only mean "have the usufruct of," not "consume." So now AASOR xvi, no. 66, line 10, egl. *ka-a-lu ak-ka-lu-mi*. Where it is applied to a sum of money, however, it seems regularly to imply the use of the capital, not merely of the income. (I am assuming, of course, that the word *usufruct* refers to the use of the income only; the legal distinction between perfect and imperfect usufruct need not concern us.)

<sup>56</sup> *Revue de l'histoire des religions*, 1934, 39 ff.

<sup>57</sup> GNC i. 128 (a footnote calls attention to the parallel with Gen. 31: 15).

<sup>58</sup> GSW 157 n.; BASOR 66. 31.



A difficulty arises at this point. In the Nuzi tablets the words *akālu kaspa* do not refer to something which is condemned, as in our Hebrew text, but to an explicitly recognized right of the person who receives the *kaspa*. In one case a father, giving his daughter in adoption to another man, says to the adoptive father, "Give my daughter Ašte to wife either to your son or in the gate; her money I declare I have consumed."<sup>50</sup> Apparently this means that the father acknowledges receipt of the full amount due from the man who is adopting his daughter, and thus renounces any claim upon her *terḥatu* when she is married. Similarly another contract provides that a woman who adopts a girl shall give her in marriage, "and forty shekels of silver for her from her husband she shall receive and consume"; a little later in the same contract the mother says that she has "received and consumed" her daughter's "money."<sup>51</sup> A third tablet says simply that the woman who adopts the girl in question shall choose a husband for her and "consume" her "money."<sup>52</sup>

All three of these cases evidently involve a form of "adoption" as a business transaction by which an entrepreneur takes over a girl, paying to her parent in advance (at a discount, doubtless) what the latter would normally expect to receive as his daughter's *terḥatu*, and acquiring in return the right to give the girl in marriage and "receive and consume" her *kaspa*.<sup>53</sup> The expression *akālu kaspa*, he it noted, is applied both to the money given to the parent by the adoptive parent and to what is received by the latter from the husband. In both cases, if it be granted that *akālu* means "consume," the person receiving the "money" is not expected to hold it as a trust-fund but may dispose of it at will as his own property.

Why then is the consumption of the *kaspa* condemned in the case of Laban? There are several possibilities. If the same customs obtained among the Aramaean ancestors of the Israelites as among the contemporary Nuzians, our Hebrew story must represent the point of view of later Israel, which no longer allowed the father to

<sup>50</sup> Note 28, no. 2, lines 23 f. Gadd translates *n-in-ut-mi* "I have accepted."

<sup>51</sup> Note 30, no. 4, lines 14 f and 27 f. I assume that Eluanza and Kanzu are the same (cf. Speiser's note in loc.).

<sup>52</sup> Note 36, no. 9, lines 9-12.

<sup>53</sup> In other adoption contracts the adoptive parent pays nothing until the girl is married; in fact, the father or mother advances a sum of money for the girl's dowry (see below, note 79).

"eat" the "money."<sup>61</sup> In that case the common view that the passage is retrojective<sup>62</sup> is confirmed and more precisely defined by the parallels from Nuzi. It may be, however, that what was regarded as legitimate at Nuzi was condemned by the Aramaeans. The charge that Laban treated his daughters like foreigners may then mean that he treated them as Hurrian fathers treated their daughters. If this be the true explanation, there is no retrojection in the passage. A third possibility is that the right to consume the *kaspu* was exceptional even at Nuzi, being allowed only under special circumstances (such as adoption) and when explicitly acknowledged in the contract.

None of these possibilities can be excluded *a priori*, but there are certain facts which favor still another hypothesis. Perhaps the story of Laban represents, not a totally different custom from that of the Nuzians, but a somewhat more advanced stage in a social process which was under way at Nuzi also. What had originally been a payment to the bride's father or guardian, becoming his property with full right of consumption, was developing into a trust-fund for the benefit of the woman, through the growth of a feeling that the father should hold part if not all of it for this purpose or give it to the girl herself. Exactly this development has taken place in Moslem lands. Arab marriage still shows traces of an earlier form in which a man simply bought his wife from her father by the payment of a bride-price (*mahr*).<sup>63</sup> At present, however, the *mahr*, or a part of it, is commonly converted into jewelry or other personal or household goods and in this form is passed on by the father to the bride herself, becoming her property.<sup>64</sup> According to Berry, a fairly large *mahr* is regarded as desirable among the Moslems of Palestine today because of the ease with which a husband may divorce his wife, both the portion given to the bride and

<sup>61</sup> Substantially this explanation was given by Prof. Julius Levy in discussing my paper at the Cleveland meeting of the American Oriental Society.

<sup>62</sup> See above, note 2.

<sup>63</sup> SKM 111 f. Writers on present-day marriage customs in Palestine and Syria take pains to show that the *mahr* is not a purchase-price. That the pre-Islamic Arabs, however, did not practise marriage by purchase is another proposition. In the *fahššiyah* many things were done which Islam condemns.

<sup>64</sup> This is not always done by the *fahššah*, but, as I judge from conversations with Arabs, it is expected among the townspeople and the well-to-do families (cf. GMC i. 135; BCE 17-18, 23-6, 31-4).

the amount retained by her father being regarded as means of providing for her support in case of divorce.<sup>67</sup>

Evidence that such a tendency was actually at work at Nuzi was seen some years ago by Speiser<sup>68</sup> in this fact: several of the marriage and adoption contracts stipulate that a sum of money is to be, or has been, bound up in the girl's *qanu* (variously translated "hem," "girdle," or "bundle"). One is reminded of the way in which the *fellāḥ* of Palestine still wears her bridal money in the form of a string of coins in her head-dress. Some of the tablets add that this money has been "released" to the girl.<sup>69</sup> The same practice appears considerably earlier in Old Babylonian tablets of the period following that of Hammurabi; here the *terḫatu* itself is bound in the bride's *qanu* and reverts to her husband if she dies.<sup>70</sup> In the Nuzi texts it is not the *terḫatu* which is bound in the *qanu* but a sum known as the *riḫtu*, i. e. "reminder." If the *terḫatu*, like the Arab *mahr*, was originally a bride-price, then the references to the *qanu* in the Nuzi tablets may reflect a process of emancipation analogous to that which the *mahr* has undergone. Such is Speiser's interpretation.

If that be so, the Assyrian Code may exhibit a further stage of this process, as Koschaker<sup>71</sup> and Speiser<sup>72</sup> have supposed. The only law in this code which mentions the *terḫatu* at all (§ 38) states that a man who divorces a wife living in her father's house may take back the "ornaments" he has given her, but the *terḫatu* "which he brought" belongs to her. If what is contemplated here is not, as we have previously seen reason to believe, a special form of marriage,<sup>73</sup> but merely a particular set of circumstances, then we may say that under such circumstances (namely, when the wife has not left her father's house or has returned to it) the *terḫatu*

<sup>67</sup> BCE 33. Cf. also GNC II, 13 n with regard to the *ḥaqq muta'addir* (postponed price), a portion of the *mahr* which is not paid unless the husband divorces the wife or dies, but is then given to her before his estate is divided.

<sup>68</sup> AASOR x, pp. 22-4.

<sup>69</sup> So HSS ix, no. 145 (GNT XXXIV); KNR no. 26; AASOR xvi, no. 15, line 16. (In the first instance Gordon translates "delivered.")

<sup>70</sup> SUA no. 209; KUH III, nos. 10, 483 (In no. 9 the *terḫatu* goes to the bride's father-in-law). SUA no. 2 simply says that the bride has received her *terḫatu* and is satisfied.

<sup>71</sup> KQU 57.

<sup>72</sup> AASOR x, p. 23.

<sup>73</sup> See above, p. 261.

is the wife's property. In other words, it has become a marriage-settlement instead of a bride-price. As a matter of fact, the conversion of the bride-price (if it was ever that) into a marriage-settlement had taken place very early among the Sumerians, as Koschaker has pointed out; the Code of Hammurabi, being under strong Semitic influence, seems to represent a relapse in the direction of marriage by purchase,<sup>22</sup> but we have seen that Old Babylonian documents of a time only slightly later state that the *terḫatu* has been bound in the wife's *qanu*.<sup>23</sup>

Speiser's view is not universally accepted. Gordon has pointed out that while the amount of the *qanu*-money and the amount paid to the guardian vary, their total is quite regularly forty shekels. The constancy of this total explains the designation of the *qanu*-money as the "remainder" (*riḫtu*), i. e. the difference between the amount paid to the guardian and forty shekels.<sup>24</sup> Gordon infers that the *riḫtu* and the *terḫatu* together formed a sum intended to provide for the wife in case of divorce or widowhood, the total amount required for this purpose being fixed by law or custom, while the portions contributed by the father and the husband respectively were variable.<sup>25</sup> According to this interpretation the wife always got forty shekels, part being provided by the husband and held in trust by the father (with right of usufruct), while the "remainder" was given to her by her father. The net cost of the whole transaction for the father (aside from his right of usufruct) was therefore the amount of the *riḫtu*. The cost for the husband was 40 minus the *riḫtu*. If this view be correct, the *qanu-riḫtu* was really a dowry, as indeed it appears to be designated in one tablet.<sup>26</sup> In favor of this also is the fact that wherever the person who binds the *riḫtu* in the *qanu* is indicated it is always the father or guardian. Sometimes, indeed, the father or mother binds the money in the girl's *qanu* before any marriage is arranged; this happens in cases of adoption, where it is provided that the future husband shall pay forty shekels to the adoptive parent, and the

<sup>22</sup> KRS 197.

<sup>23</sup> See above, p. 271.

<sup>24</sup> The word is not used exclusively in this way; in HSS v, no. 11 (AASOR x, no. 31), line 27, the *riḫtu* appears to belong to the adoptive parent (unless *še ana Ma-et-ka, šar* means "who belongs to Matkašar").

<sup>25</sup> GSW 167.

<sup>26</sup> HSS v, no. 60 (AASOR x, no. 26); see below, p. 275.

letter shall return to the father or mother the amount of forty shekels minus the *qannu*-money.<sup>79</sup>

Gordon's theory is open to certain objections. Why the amount provided for the wife should be fixed, while the portions contributed by the husband and the parent respectively varied within this amount, is hard to see. That the sums given should vary according to the wealth of the parties concerned and the age and desirability of the girl is comprehensible, but one would then expect that the total provision for her also should be variable. Apart from *a priori* considerations, we know that the bridegroom was often, if not regularly, expected to pay forty shekels. One tablet mentions the amount of forty shekels as "the price of a daughter of Arrapha."<sup>80</sup> In a marriage-contract of the simplest kind<sup>81</sup> a man declares that he has given his sister in marriage to a man named Hurazzi and adds, "and from Hurazzi forty shekels of silver for my sister"<sup>82</sup> Belit-Akkadi-Ummi I have received and am paid." In still another case a man who adopts a girl as daughter-in-law in order to marry her to his own son pays forty shekels of silver as her *kaspu*.<sup>83</sup> On the basis of Gordon's theory we should have to suppose that where the husband pays the full forty shekels there is no *qannu-rêtu* payment from the father. If the husband, on the other hand, gave no *terêtu*, the father would presumably be obliged to provide the whole sum. This, in fact, is what Gordon assumes, for he states that if Jacob had paid nothing at all for his wives, Laban should have given a dowry equal to the normal amount of the bride-price plus the dowry.<sup>84</sup> Since the sources furnish no example of such a case, the assumption is at least open to question. In any case, the fact that the husband so often pays the full amount remains an obstacle in the way of Gordon's theory. It is hardly likely that some fathers would give nothing at all if others gave the greater part or all of the forty shekels.

<sup>79</sup> So, with some variations, nos. 8 and 10 of note 36, above, and AASOR xvi, no. 55.

<sup>80</sup> Note 36, no. 1 (Gadd's transliteration): 40 SU kaspu za mârât ar-ra-ab-ê. Gadd's translation, "40 SU of silver for a girl of Arrapha," misses the point and does not fit the rest of the sentence.

<sup>81</sup> Note 36, no. 6, lines 7-10.

<sup>82</sup> Or "40 shekels, the *kaspu* of my sister" (40 SU kaspu za a-bê-ti-ia).

<sup>83</sup> HSS v, no. 79 (AASOR x, no. 25).

<sup>84</sup> GPN 3 n.

Another explanation has been suggested to me by Prof. Goetze.<sup>22</sup> Originally the *terḥatu*, amounting normally to forty shekels, was paid by the husband to the father and became the latter's property. It was also customary for the father to give his daughter a dowry, the amount being something less than that of the *terḥatu*. In the course of time these two payments were combined. Instead of giving forty shekels to the father, the husband now paid merely the excess of this amount over the amount of the dowry and bound the "remainder" in the bride's *qanu*. The father thus made no actual gift directly to the bride but allowed what he would have given to be deducted from the amount he received from the bridegroom. The term *terḥatu* was now used for the sum actually paid to the father instead of the full amount. To "eat up the money" then meant to take the full amount, not allowing the wife any *riḥtu*.

This interpretation allows for the fact that the *terḥatu* plus the *riḥtu* amounts to forty shekels. What is constant, however, is the amount for which the husband is responsible, the amount received by the father and the amount of the *qanu-riḥtu*-dowry being variable. The objections to Gordon's theory are thus avoided. New questions, however, are raised by this explanation. The assumption that the husband gives the *qanu-riḥtu* to the wife runs counter to the fact that in the sources it is the father who declares that he has done this or will do it. The answer to this objection may be that while the husband makes the actual payment to the wife, he does not determine the size of it, nor does it affect the total amount for which he is responsible, whereas the father or guardian determines how large the *qanu-riḥtu* shall be and by virtue of accepting a correspondingly reduced amount himself remains the real giver of what the bride receives. Accordingly it is he who makes the declaration, thus giving in effect a quitance for the amounts due both to him and to his daughter. Or it may be that the husband paid the forty shekels to the father, and the latter himself took from it the amount of the dowry he wished to give his daughter. In this case the only difference between Goetze's and Speiser's theories is that by the former the *riḥtu* replaced an earlier dowry, whereas Speiser's view implies either that there had been no dowry before the *riḥtu*-practice arose or else that the dowry remained a separate matter and was still given in addition to the *riḥtu*.

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<sup>22</sup> In a private communication.

The word *mulugu*, commonly translated "dowry," has been noted in one tablet.<sup>88</sup> The syntax of the passage is not certain, but the meaning appears to be that the *rihtu* is bound in the *qanna* for a *mulugu*.<sup>87</sup> Both at Nuzi and elsewhere *mulugu* regularly means a gift from a father to his daughter.<sup>88</sup> This gives us no basis for determining the origin and nature of the *rihtu*, however. By any of the theories we have noted the *rihtu* may be called a dowry, since it is given to the bride by her father or guardian, whatever its origin.

In the Old Testament there are references not only to the *mohar* given by the husband but also to a gift from the father to his daughter. The Hebrew name for this is *šillūḥim*. Dussaud maintains that it was given, as the name suggests, when the bride was sent away from her father's house, and that Laban, by keeping his daughters and Jacob with him, was evading the necessity of making this parting gift.<sup>89</sup> This interpretation rests upon the theory that Israelite marriage was essentially an alliance between two families through the union of the young couple, the transaction being closed by an exchange of gifts. Dussaud regards the *šillūḥim* as the "compensation" for the *mohar*, and vice versa. The result of Laban's failure to give the *šillūḥim* and let his daughters go, he infers, was that Jacob's labor was not a true *mohar* but a mere purchase-price, since there was no "compensation"; hence the charge of the daughters that their father had sold them.<sup>90</sup> But we have seen that Jacob's remaining in Laban's house corresponds to a recognized form of marriage in the ancient Near East; this part of Dussaud's explanation therefore falls to the ground. It still remains possible, of course, that, as both Dussaud and Goetze suppose, Laban had not only retained the *mohar* but had also failed to give the *šillūḥim*. There is nothing to suggest this, however, in the language attributed to Leah and Rachel. The maids given by Laban to his daughters<sup>91</sup> may in fact represent the *šillūḥim* or part of it.

Gordon's and Goetze's interpretations of the *rihtu* differ alike

<sup>88</sup> Note 36, no. 7.

<sup>87</sup> So Speiser's translation. The text reads: (11) *mi-xu-um-me-e* (12) *ka-sap-tu ri-ih-tu la* (*Be-el-ta-ak-ka-du-um-me*) (13) *a-na me-du-gu-ti* 4 *a-na qe-an-ni-tu* (14) *a-na* (*Be-el-ta-ak-ka-du-um-me*) *ir-ta-ak-si-mi*.

<sup>88</sup> For references and discussion of the term cf. KRS 175; KE 20 n; AASOR x, 24; GSW 157 f; BASOR 65, 36.

<sup>89</sup> DMI 148.

<sup>90</sup> Ib. 149.

<sup>91</sup> Gen. 29: 24, 29.

from Speiser's in that they involve a dowry given independently by the father as an element in the transaction. Both, moreover, identify this dowry with the *riḫtu*, the difference being that Gordon thinks the husband paid less than forty shekels and the father provided the "remainder," whereas Goetze believes the husband paid the full amount, giving part of it to the father and the "remainder" to the bride, so that the father only gave the dowry indirectly. Speiser's theory makes the whole matter a development in the conception and use of the *terḫatu* itself, in such fashion that a part of the *terḫatu*, and ultimately all of it, was converted into a dowry through being passed on by the father to his daughter, as in Islam.

On the whole, while the facts at our disposal hardly permit a conclusive demonstration, the choice would seem to lie between the theories of Speiser and Goetze. Of the two I am inclined to regard the former as more probable. For our present purpose, fortunately, a final decision on this point is not necessary. Whether the bride-price and dowry, originally independent, had been combined in a single transaction, or the bride-price itself was on the way to be converted into a dowry, it is clear that in the mixed Hurrian and Semitic culture of northeastern Mesopotamia, at about the time in which Laban and Jacob are supposed to have lived, a part of the money paid by a bridegroom for his bride was commonly delivered to the girl instead of being regarded as her father's property. It follows that the evident reference to some such custom in the complaint of Laban's daughters need not be thought of as a retrojection of later Israelite ideas. Indeed, the close relationship in terminology between this passage and the Nuzi documents strongly suggests an underlying relationship in tradition and social practice.



## TODA GARMENTS AND EMBROIDERY

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THE GARMENTS worn by Toda men are three in number: the [kɯm:n] or perineal cloth, the [torp] or waistcloth, and the [pu:txuŋj] or cloak. Women wear the latter two. Rivers<sup>1</sup> has given pictures of the various ways in which the [pu:txuŋj] is worn. Men wear the [torp] by carrying one of the long edges completely round the waist and tucking the end of this edge in at the left side, letting the garment fall straight to the knees (Rivers, fig. 15, p. 51). The garment is adjusted so that it falls only as far as the knees by folding over a sufficient quantity of material at the top before carrying it around the waist. Women wear the [torp] by passing it in the same way around the body under the armpits; in Rivers' fig. 11, p. 33, the standing woman is wearing a cloth in this way with both arms free. Children until puberty wear only a [torp], draping it as their elders do the [pu:txuŋj].<sup>2</sup>

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<sup>1</sup> W. H. R. Rivers, *The Todas*; London, Macmillan and Co., 1906; see illustrations *passim*. For the phonetic writing employed in this paper, see my paper on *The Phonemes of the Toda Language*, which will be published in *Language*. One modification is made in this paper from the system used there, viz. | denotes the voiceless retroflex lateral phoneme. I must express my gratitude to Miss F. Sidney Smith of Ootacamund, who has given me much help in my study of the embroidery.

<sup>2</sup> The [torp] is a garment adopted only in comparatively recent times. Finicio's account of his visit to the Todas in 1803 (Rivers, 721-730) is very explicit about the clothes worn then. On p. 726 he says: "they [the men] are clothed in a large sheet with no other covering but a small loincloth four or five inches wide." These must be the [pu:txuŋj] and the [kɯm:n]. One often enough sees a Toda without suspecting that he is wearing a [torp], so well is it covered by the cloak, but the [kɯm:n] is so seldom visible when a Toda is completely clothed that we can be sure that Finicio's enumeration is complete. Of the women he says (p. 727): "the women wear nothing but a long sheet like the men." This is the [pu:txuŋj]. The account which follows makes it clear that the women present were keeping themselves very modestly covered, as they do at the present time, though it is hard to believe that Finicio might not have discovered by observation alone that a [torp] was worn under the cloak, if it was indeed worn. But an incident in one of the stories that I have recorded makes it clear that Finicio was correct and that women did not wear the [torp] until comparatively recent times. The story was told me by a young man of his

The material of the garments is khaddar, that is, a rather heavy, somewhat coarse, hand-woven cotton material, almost white in color. It is made to order for the Nilgiri tribes by Chettis in Coimbatore and Satimangalam and probably other towns in the plains on the Tamilian side of the Nilgiris and is brought up to the hills by

great-grandfather's wife who was divorced by him because at a funeral she opened her [pu:txu:] by grasping one end in either hand and shook it to remove the dust. In doing this she uncovered her body completely and outraged her husband's sense of decency. This incident cannot have happened less than a century ago.

Two of the oldest men of the tribe have independently given accounts of the adoption of the [torp] by men. They are [pə:əə:] of [ka:g] (Rivers' Peithel in table 7), and [ki:nuz] of [ke:re:ɾ] (Rivers' Kiner in table 20). In the days of their grandfathers only [pu:txu:] and [kəw:ə] were worn; in their fathers' days men began to wear as waistcloths pieces of worn-out cloths, called [okent], and then the [torp] of khaddar. In the youth of these old men the [torp] was worn, but not universally, and [ki:nuz] has never worn this garment, following the old custom. He is however at the present time the only man whom I have discovered who does not wear it. Both of these men are of a great age; [pə:əə:] claims to be over 100 years old, [ki:nuz] is more moderately content with 90 years and pours scorn upon the claims of [pə:əə:] to be older than himself. [ki:nuz] says that he has seen the [pju:f] in bloom five times and that he was born in a year when it was in bloom. The [pju:f] or [piu:fxot] is a species of the genus *Strobilanthes* ([kət]), most of which are multicaulic; since 1838 is not a year of abundant flowering, I have been able to get specimens of the leaves only and from these the species cannot be exactly identified. The exact periods of the blooming of the various *Strobilanthes* species have not been very accurately observed. It is usually said by the Todas that this species blooms every eighteen years, though one man has told me that in former times it bloomed once in eighteen years, but now once in about 14 or 15 years. [ki:nuz] is obviously 75 years of age or older; how much older it is impossible to say. Neither [ki:nuz] nor [pə:əə:] remembers his grandfather. It is clear then that the Todas began to wear the [torp] about a century ago.

A further line of investigation suggests itself in the accounts of the Todas by early English visitors to the Nilgiris. The earliest accounts are, in appearance at least, explicit enough (I have not been able to see Harkness, *A Description of a Singular Aboriginal Race Inhabiting the Summit of the Neilgherry Hills*; London, 1832. Extracts that I have seen seem to agree with the accounts that follow). Lieut. Evans Macpherson in his letter of 1820 to John Sullivan (H. B. Grigg, *A Manual of the Nilgiri District*; Madras, 1880; appendix, p. lvi) speaks of "a single cloth" as being worn by both sexes. B. S. Ward in a memoir of 1821 (the same volume, appendix, p. lxxiv) mentions [pu:txu:] and [kəw:ə] for men, [pu:txu:] only for women. So also Ouchterlony (*Geographical and Statistical Memoir of a Survey of the Neilgherry Mountains*, 1847, published in *Madras Journal of*

Chetti middlemen. That used for the [torp] is woven in three sizes: two cubits<sup>2</sup> and one [kud] by one cubit and a span, three cubits by two, and three cubits and one [kud] by two cubits (the latter size I have not seen and cannot describe; it is rarely used now). The long edges of all three sizes are woven with a selvage; the shorter edges are unfinished. At the present time this material for the [torp] has been displaced by cheaper, machine-made cotton; a very few old men are seen wearing it, and the [torp] put on a dead person and burned with the body is usually of the old style. When the khaddar material was generally used, the three by two cubit size was that most usually worn; the smallest children used the smallest size.

The [pu:txu]j] is always made of khaddar. When completed it measures three cubits wide by five and a half cubits long (occasionally six), and is of double thickness. This is obtained by laying together two strips ([kut]), each one and a half cubits wide and eleven (or twelve) cubits long, folding the short edges together so that there are now four thicknesses of cloth, then basting together the four thicknesses with dark-blue or black thread ([ka:g]) about

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*Literature and Science*, vol. 15 (1848), on p. 51) mentions a single cloth as the dress of the men. All this seems conclusive for the absence of the [torp] as late as 1847, but what can we make of W. E. Marshall's account (*A Phrenologist amongst the Todas*; London, 1879; p. 49) that the men wore only [kpu:ɔ] and [pu:txu]j], when in the photograph given as a frontispiece to his book the man on the extreme left is obviously wearing a [torp]? Was he repeating the previous accounts? And, if so, how many of the previous writers (Ochterlony especially is suspect on other grounds as well; see Rivers 477) repeated the accounts of their predecessors? It is only when we come to Rivers that fresh observations were made and the [torp] was mentioned. These accounts throw no very certain light; they merely confirm the approximate dating that results from the Toda tradition.

This history of Toda fashion explains in an interesting way a minor feature of the Toda songs. These show a rigid parallelism of conventional pairs of words. In their occasional references to clothes the [torp] is not mentioned either for men or for women. [tɔw:ɾɪxu:c], whose meaning as an embroidery term is explained below, in the songs means "embroidered [pu:txu]j]"; its pair is always a word meaning "earrings." The pair was formed before the [torp] began to be worn, and has not been altered by modern fashion.

<sup>2</sup> The cubit [mɔgɔj] is the length from elbow to finger-tips, about 18 inches; odd lengths are given as [ɔwɾɪxu:d], lit. "one-fist," the length from elbow to knuckles of the closed hand, about 14 or 15 inches, or in spans [kɔw:ɔ], i. e., the distance between tips of thumb and little finger when the hand is outspread, about 9 inches. 2 spans = 1 cubit.

6 cms. in from one of the long edges, and then turning the outer length of material away from the inner so that it forms one longitudinal half of the garment while the inner strip forms the other half. Colored stripes (which will be described later) are woven in the cloth; they are more elaborate at one end of the eleven-cubit strips than at the other. In the sewing these more elaborate ends have been kept together and the stripes now form continuous bands across one end of the cloak, while the less elaborate stripes also are continuous across the same end on the reverse side which is the inner side of the garment as worn. More sewing is now done. If the garment is hung up with the fold at the top, the seam of the first sewing down the middle, and the elaborate stripes at the bottom, the open edges at the top right-hand corner are basted together for a distance of one [kud] down from the corner. The pocket thus formed is the [kyu:s] in which articles are carried (Rivers, 571). The lower open edges of the hung-up cloak are basted together for about a cubit on either side of the central seam, the stitches being made about 4 cms. in from the edge; at the lower left-hand corner similar rows of stitches are started at the corner and carried, one about half a cubit along the lower edge (this is sometimes omitted) and the other a [kud] up the left-hand edge and very near the edge, until it meets the first woven band. The sewing along these edges has no other object than to keep the two thicknesses of the garment together and make the whole more easily managed when it is worn. All this last sewing is finished at the ends with large knots of the thread, which are clearly seen in fig. 10; they are called [mu:rjfa:kjfoj], a word whose analysis is still uncertain.

Both types of cloth, for the [torp] and for the [pu:txulj], have colored stripes woven in. Those of the smallest type of [torp] are as follows. Along the selvage at both sides and one cm. from the edge run two stripes one mm. apart. The outer is of crimson with two orange lines on either edge separated from one another and from the edge by crimson threads; the whole stripe is one cm. wide. The inner stripe is of alternate lines of crimson and orange. Across each end run three groups of stripes. From 15 to 17 cms. from the end are two black stripes, each 6 mms. wide and one mm. apart. Between 5 and 6 cms. in from these is a stripe one cm. wide of crimson with two orange threads dividing it along its length into three sections of equal width. Another 5 or 6 cms. in comes another

group of stripes, comprising a central one 38 mms. wide of crimson with a stripe like the last described at a distance of 5 mms. on either side. All these stripes run to the selvage and eliminate the stripes running along it. The larger [torp] cloth has the same general patterning of stripes, but with differences of detail. The stripes along the selvage are two with these measurements: distance from edge 15 mms., width of outer stripe which is of a deep coppery red 15 mms., distance between stripes 2 mms., width of inner stripe which is orange and the coppery red in alternate threads 18 mms. At each end and about 33 cms. in from the ends are two black stripes each one cm. wide and 7 mms. apart; the outside edges of the pair are crenellated, as are both edges of each of the three remaining stripes to be described. About 7 cms. in from the black stripes is a stripe 25 mms. wide of the coppery red divided into three equal longitudinal sections by two orange threads. About 6 cms. in from this is a similar stripe and another 6 cms. in is a third. In weaving the stripes on both these types of [torp] those across the ends are woven double so that they are of solid color; those along the selvages are woven single and the white of the background blends with the colors to give somewhat lighter shades of the colors. The same is true of the corresponding stripes on the [pu:txuŋj].

The woven stripes of the [pu:txuŋj] are as follows. Each of the long strips one and a half cubits wide has along both selvages, between 2 and 3 cms. from the edge, two crimson lines slightly more than one mm. apart. The more elaborate ends mentioned above have at a distance of one [kud] from the end a broad black stripe about 8 cms. wide. About 16 cms. in from this is a stripe of solid crimson 75 mms. wide; another 12 cms. in is a stripe of solid crimson about 8 cms. wide. Between the black stripe and the first crimson stripe, about 10 cms. from the black, at either side is an isosceles triangle of single-woven crimson with its base of 25 mms. resting on the edge of the cloth and its perpendicular about 35 cms. Similarly between the two crimson stripes and about 6 cms. from the first is a triangle, with base 25 mms. and perpendicular 35 cms. The sides of these triangles are not straight lines, as the nature of the weaving prevents this, but at intervals of some cms. are staggered inward until they finally meet at the apex. At the central seam of the completed garment the triangles unite at their bases to form very elongated diamonds. The less elaborate ends, which are not visible when the cloak is worn, have stripes: about 13 cms. from the

edge a black line and one mm. in from it a black stripe 5 mms. wide. About 18 cms. further in is a similar group in crimson, and about 8 cms. in is a solid crimson stripe one cm. wide.

All these measurements are to be taken as approximate. As the weaving is not machine-done, there is much variation, but all within the general patterning described. Another type of [pu:tru:] frequently seen has the stripes on its elaborate end with the following measurements: one [kud] from the end a black stripe 3 mms. wide and at a distance of 3 mms. from it a black stripe 42 mms. wide. At a distance of 17.5 cms. in from this is a crimson stripe 45 mms. wide, and 10.5 cms. in is a crimson stripe 55 mms. wide. The triangles of the same measurements as on the other cloak are respectively 11 cms. in from the wide black stripe and 35 mms. in from the first crimson stripe. It will be observed that the stripes are much less wide than on the first-described cloak; in fact this is not a Toda cloak at all, but that worn by Badagas and preferred by them to the Toda type. It is somewhat cheaper than the genuine Toda article and is worn by Todas only when they are unable to afford the type which they prefer. Kotas also are to be seen on ceremonial occasions (but not when they are playing music at Toda funerals) with cloaks of this general description; I have had no opportunity to examine them closely, but a striking variation is a green stripe, and it is clear that the general patterning is rather different from that of the Toda or Badaga types of cloak. Kurumbas are also occasionally seen with a cloak of Toda or Badaga type, but this is not their usual dress. To anticipate what follows, the embroidery which is done by Toda women for Toda wear is also done by them, though with some scamping of detail and material, for the more important Badagas at a price of six to eight rupees. Kota cloaks have what looks like embroidery, but I am informed that it is a woven pattern.

The shades of coloring of the stripes on [torp] and [pu:tru:] vary considerably because of non-standardized methods of dyeing. The color which I have called crimson varies much in intensity, but only one color seems to be intended. As the [torp] is rather rarely seen, there has been no opportunity to observe variation in the color I describe as a deep coppery red. The orange color is found only in the [torp] and is slightly different in the two specimens in my possession. Some of the variation, especially in the reds, is undoubtedly due to washing, as the dye is not fast.

Perhaps the most striking feature of Toda clothes is the embroidery done by the Toda women. Most cloaks that are seen have a small amount of needlework on them. The best and most elaborately worked specimens however are worn by men and women of some importance and affluence, as well as at festive gatherings, including funerals, by all who are fortunate enough to possess them, and elaborate specimens are kept stored away by elderly people to be put on their bodies when they are dead. The most elaborate specimens of [torp] are now kept for this latter purpose. It must be noted that the [torp] ordinarily worn, that of machine-made cotton, has usually no embroidery on it, except sometimes a simple edging.

The only technique used in the embroidery is that of darning and counting the threads of the woven material to form the patterns. The verb stem [o:tj-] is used of the work; the product is [o:tjfoj] "which (she) embroidered." A few simple patterns are used as the basis of all the work. They may be divided into two main classes. The first is employed in the spaces between the broad woven stripes on the elaborate ends of the cloak and between the woven stripes on the ends of the [torp]. It will be seen from the drawings of these patterns and from the photographs of specimens that the side of the material on which the patterns are most clearly seen is taken by the Todas as the wrong side or reverse of the work, while the side with the loops of the thread made each time the direction of the darning is reversed is the right side or obverse. The thread used, called [ko:g], is a coarsely spun thread of 3 or 4 strands (obtained in the bazaar), usually dark-blue ([pu:tʃa:g]), occasionally black ([karjɔ:g] "charcoal-thread").<sup>4</sup> The heavy effect of the generous loops of this thread on the obverse seems to be the deciding factor in the choice of this as the right side. The patterns used in this class of work may be divided again into three subclasses. The first is made up of the pattern called [mi:tʃu:fjɔp] "eyes ([kɔp]) on the peacock's ([mi:tʃ]) tail-feathers ([tu:fj])," and its elaborations. Fig. 1 shows the pattern with the smallest number of central dots. These may be indefinitely multiplied, keeping always to the diamond shape; when the number of white

<sup>4</sup> Occasionally some of the work is done in pale blue or in crimson thread; but this is due to foreign influence (see note 13). One cloak that I have seen is worked in a light bluish-gray, which contrasts effectively with the crimson and black of the woven stripes.



spaces counted from loops to loops perpendicularly through the center of the diamond reaches seven, the pattern is called [kop-aŋ] "butterfly." The pattern is usually extended as in the figure, but frequently a line of the primary elements is made at right angles to the line of extension in the figure, and is used both on [pu:txulj] and [toɾp] inside of the woven stripes on the edges (see fig. 13).

The second is the [moɽu:rjɸuxu:r] "mat-embroidery," indefinitely extensible in one direction only (fig. 3). The interior lines of dots in this pattern vary considerably in number.

The patterns of the other sub-class (figs. 3 and 4) are all made on the same principle, that of two zigzag rows of loops enclosing any number of corresponding zigzag lines. The stitches of the interior zigzags on the obverse side always extend over at least two of the threads of the woven material. On the reverse the stitches extend over one thread or more; in consequence, the obverse and reverse have a quite different appearance. The patterns are differentiated on the basis of the number of horizontal white threads enclosed within each line of loops (between every two loops there is only one white thread). If there is only one white thread, i. e. two loops (fig. 3), the pattern is called [aɸilɸuxu:r] "squirrel-embroidery" ([puxu:r]),<sup>5</sup> through a fancied resemblance to the stripes on a squirrel's back. That with two white threads, i. e. three loops (fig. 4), is called [po:ɸɸuxu:r] "snake-embroidery," or [æ:ɖaŋkɸɸuxu:r] "two-grains of rice-embroidery." Each white thread ([nu:s]) counted is called [aŋkɸj] "grain of rice." Those with three and four white threads are respectively [mu:ɖaŋkɸɸuxu:r] "three-grains of rice-embroidery" and [no:ŋaŋkɸɸuxu:r] "four-grains of rice-embroidery." That with five threads is [yɖaŋkɸɸuxu:r] "five-grains of rice-embroidery," though instead of this name [utqunɖɸuxu:r] "big ([ut])-which is ([qunɖ])-embroidery," or [tɸu:rjɸuxu:r] "pair-embroidery,"<sup>6</sup> is more frequently used. Another pattern, called [poɖuŋkɸɸuxu:r],<sup>7</sup> not very often used, is shown in fig. 4a, and is

<sup>5</sup> Why a pair is referred to is unknown to my informants, and I can see no reason.

<sup>6</sup> [poɖuŋk] is a species of snake with spotted skin, which gives the embroidery pattern its name. This pattern is used only on garments meant for a dead person, and is said to prevent them from being taken from the spirit on its way to the world of the dead by [kəŋ], the messenger of [æ:n], the god who lives in the afterworld. I have been unable to find out any reason for [kəŋ]'s distaste for the [poɖuŋk] pattern.



to be classed with those last described. It differs from them in that the interior zigzag lines are one thread wide and one thread apart, so that the reverse is almost identical in appearance with the obverse. All these patterns are indefinitely extensible in the one direction only.

The manner in which the patterns are used and combined is seen in fig. 10. Between the two broad crimson stripes at the left side of the photograph are four long strips of embroidery, extensions of the [yðəŋkjɸuxu:r] pattern. At each end of these strips and at the edges of the cloak three strips (called [oðɸuxu:r] "crosswise-embroidery") are worked at right angles to the woven and worked strips. The patterns used are [pə:ɸɸuxu:r] for the outside strips and [moðæ:rɸɸuxu:r] for the strip between them. To the right of the second crimson stripe are three strips of embroidery, two of [moðæ:rɸɸuxu:r] enclosing between them one of [pə:ɸɸuxu:r]. At the ends of these are three strips of [oðɸuxu:r], of the same patterns as the last described [oðɸuxu:r]. Most embroidered cloaks, and [torp]'s also, show similar combinations of varying numbers of long strips of embroidery of various patterns. Sometimes a long strip is divided up into several sections of different pattern. The [oðɸuxu:r] may or may not be present.

Two types of stitch, not included in either of the two main classes, are found on most garments. In the cloak in fig. 10, on either side of the black woven stripe is a line worked in whipping stitch. In the photograph the one to the right is visible; the other is somewhat obscured by a row of work to the left of it called [otstəŋtɸə] or [ɸurɸkjɸə].<sup>7</sup> This is also to be seen at the lower edge of the cloak and at both ends of the [torp] in figs. 11 and 12. It is primarily used in these places to prevent the unfinished ends of the material from ravelling, and is made by darning with a thread made of a strand of [kə:g] and one of crimson thread. The remainder of the work on the cloak belongs to the second class of work, now to be described.

The second main class of work is found between the ends and the first colored woven stripes of [pu:ɸɸulɸ] and [torp]. It is worked with single strands of [kə:g] and of crimson thread, and is much

<sup>7</sup> The former means "that which seized the edge ([otw])." The second is from the verb stem [ɸurɸkj-] which denotes this kind of work; it means "which (she) worked in this way."

flatter and more delicate in character than the type of work last described. It, like the other, is done by darning and counting threads, but unlike the other, the darning is done along the length of the strip of pattern and not across it, so that there are no loops of thread at the edges of the strip and the pattern is alike on obverse and reverse. The strips of pattern run at right angles to the woven stripe, and usually extend from it to the very edge of the cloth. Again, the work falls into two types. In the first (figs. 5-8) threads of the woven material are drawn, and a number of threads is darned into the space left. So in the [kəŋkotjxorj], one thread is drawn and two threads are worked in; in the [tagas] "chain," two threads are drawn and four worked in so as to form three lines; in the [no:ndyljxorj] "four holes in the gate-posts of the pen" ([no:n-] "four," [tyj] "gate-post of the pen," [kon] "eye," here "hole for the pen-bar"), three threads are drawn and five worked in four lines (whence the name). There is also an [yds@yljxorj] "five holes in the gate-posts of the pen," in which four threads are drawn and six worked in, the center line being made up of two threads. An edging is used called [mam@yljfoj],<sup>8</sup> in which one thread is drawn and two are worked in to form one continuous line; this is usually done in crimson, and may be used to edge e. g. a dark-blue [yds@yljxorj]-pattern or a pattern of [o:xofæ:sj] (see fig. 9). Sometimes the [yds@yljxorj] is made with the four center threads in dark-blue and the two outside threads of crimson.

The second type of this work is the [o:xofæ:sj],<sup>9</sup> in doing which no threads are drawn from the woven material before the colored threads are worked in. The thread used may be either dark-blue (or black) or crimson. The most usual pattern made in this way is shown as the first in fig. 9; there it is edged by the [mam@yljfoj]. On the [pu:trulj] where larger designs are desired a third row may be added to form a zigzag or the design may be doubled; frequently one row only is worked on both sides of a design of the last group.

<sup>8</sup> Only the second element of the word is clear; it is from [tytj-] "to twine or make in loops." The whole word is used also for the strips of cane twined around the thatch at the ends of the roof of the house or dairy.

<sup>9</sup> The word means "wall of pen ([pæ:sj])—which is not ([o:xo-])." The exact force of the word is not clear, but probably something in the appearance of the gap made when threads are drawn in the other type of work reminds the Tudas of the wall of a pen, and when threads are not drawn we have a non-existent pen-wall.

usually in the contrasting color. Sometimes this work is done in alternate threads of blue and crimson. In fact, combinations of the drawn-thread patterns and the possible varieties of [o:xofæ:sj] are infinite in number. It would be wearisome to give details of the patterns employed on the cloak in fig. 10, since all the stripes except those on the ends are done in slightly differing varieties of [o:xofæ:sj]. The end stripes are each made up of five [tagas], the center one being made to fill exactly the space between the woven crimson lines that run the length of the selvage.<sup>10</sup>

The [torp] that is photographed obverse and reverse in figs. 11 and 12 is a particularly elegant and tasteful specimen of work more elaborate than usual. Details of the work between the woven stripes may be seen in figs. 13 and 14, and space need not be used to describe them. Details may be given for the patterns between the first woven stripe and the end of the cloth at the bottom of fig. 12. All these are done in black and crimson. Only the crimson will be mentioned; otherwise it is understood that the thread used is black. Between the left edge and the longitudinal woven stripe there are two [tagas], and inside the longitudinal woven stripe three [tagas]. Then a strip made up of one [no:ndy]jxon], two [tagas] separated by crimson [o:xofæ:sj] of five threads making a zigzag (see the second pattern in fig. 9), and one [no:ndy]jxon]. Then a strip made up of two [no:ndy]jxon], each edged on both sides by crimson [o:xofæ:sj] of two threads each, and separated by a [konkotjxor]. Then a strip the same as the last but one, but with the crimson threads forming a herring-bone pattern. Then a strip made up of two [ydgŷ]jxon] whose outside threads are crimson, each edged on both sides by [o:xofæ:sj] of two threads each. Then a strip made up of an [o:xofæ:sj] of the herring-bone type, edged with crimson [mu]ŷy]foj], and with two [tagas] on either side. Then a strip made up of a similar but broader [o:xofæ:sj] with its herring-bones

<sup>10</sup>The only remaining element of decoration on the cloak photographed is the row of fringe ([kywe]). In this specimen the fringe is made of crimson threads each terminating in two or three cheap glass beads of varying colors, silver, yellow, and green, and very fragile. This cloak, before I acquired it, was stored away and intended to be worn by a dead person; hence the unpractical nature of the ornaments. On one cloak that I have seen, the fringe is continuous along the edge and is made of heavy white parcel cord. One may guess that fringe is not an essential part of the garment and is probably an innovation.

pointing in the opposite direction to those of the last, with the same crimson edging, and on either side a [no:ndy|jxon]. This is the center strip, and the rest of the strips repeat those already described in reverse order. We have here perfect symmetry, and the same is true of the other end of the [topp], though the patterns there are entirely different from those just described. The combination of patterns between the woven stripes is one of considerable complexity, each of the six bars of embroidery having a different combination. In each of the bars there is a very close approximation to symmetry, what failure to achieve it there is being due to difficulty in dividing up a long space beforehand by counting the threads exactly. I am assured by Todas who have seen this [topp] in my possession that it is a specimen of more complexity than is usually seen, and that its fame has spread abroad and caused some rivalry among the women who are especially skilled in needlework.

It has been said that the "artistic side of life among the Todas is but little developed."<sup>11</sup> This statement is true in a comparative sense; we can recognize only three arts that have been developed at all. The art of song is the chief of these and absorbs to a large extent the energies and capabilities of many individuals of both sexes. Of material arts, a rather primitive pointillist ornamentation of wooden articles is frequently seen; but the material art par excellence is that of embroidery. It is practised by most of the women and preeminence in the art is recognized and gives a woman distinction.<sup>12</sup> We have seen that only one very simple technique is employed. The results obtained with this by the best workers are in the highest degree attractive. The more delicate type of patterns

<sup>11</sup> Rivers, 570.

<sup>12</sup> Both the garments photographed for this paper were embroidered by [sondori:dz], a woman of about 30 years of age. She is too young to be included in Rivers' genealogies, but is to be fitted into table 63, as the daughter of [midxpu:r] (Rivers' Midjkudr) of the [pi:to:] and his wife [ko:sar]. While I cannot say with any certainty that she is the best embroiderer of the tribe, she ranks very high, and in addition is one of the best of the composers of songs, accepted as such by the male composers. Her intelligence is of a remarkably high level; her powers of speech have been fully recognized in the nickname [pakiljfa:] "lawyer-flower." ([pakilj]) is the Toda adaptation of the Tamil [vak:il]; the i-quality of Tamil [i] described by J. H. Firth, *Short Outline of Tamil pronunciation*, p. xiv, in Arden, *Progressive Grammar of Common Tamil*, 4th ed., is reproduced by [j] in the Toda word; Toda [i] has not the i-quality of the Tamil.)



Fig. 1. [nū: (n: f) xon]

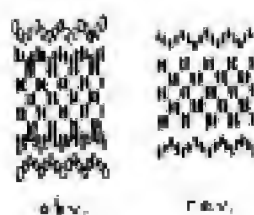


Fig. 2. [molar: r] fuxu: r]



Fig. 3. [aŋli: xon: r]



Fig. 4. [po: h fuxu: r]

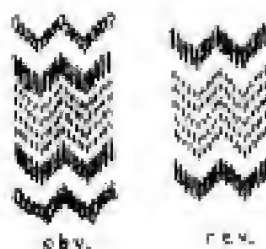


Fig. 4a. [po: h xon: r]



Fig. 5. [kəukot] xarj]



Fig. 6. [tagu: r]



Fig. 7. [u: n d y] xon]



Fig. 8. [yidəy] xon]



Fig. 9. Two types of [o: xofə: s], the first with [u: w] [əy] [foj]



Fig. 10. End of (p; axu) with woven stripes and embroidery

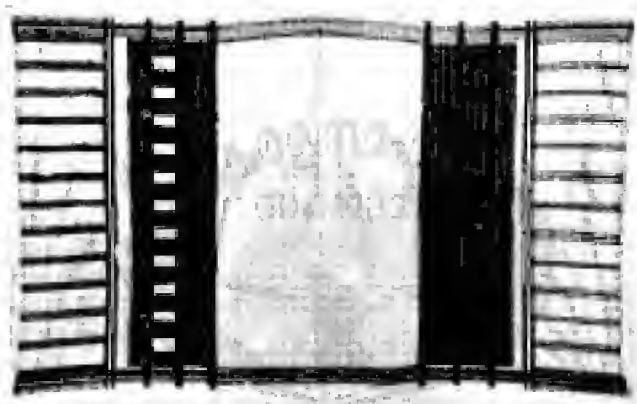


Fig. 11. Obverse of (top)

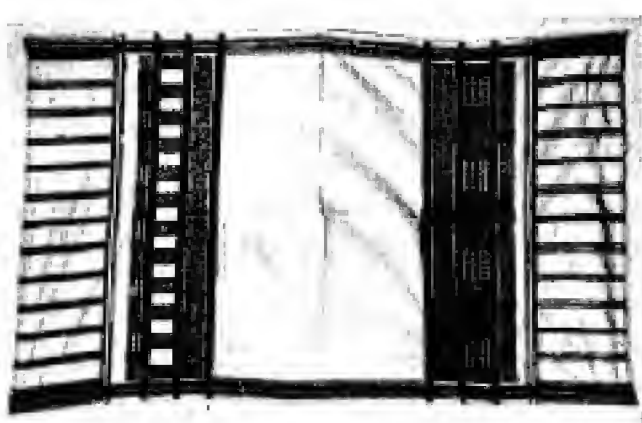


Fig. 12. Reverse of (top)

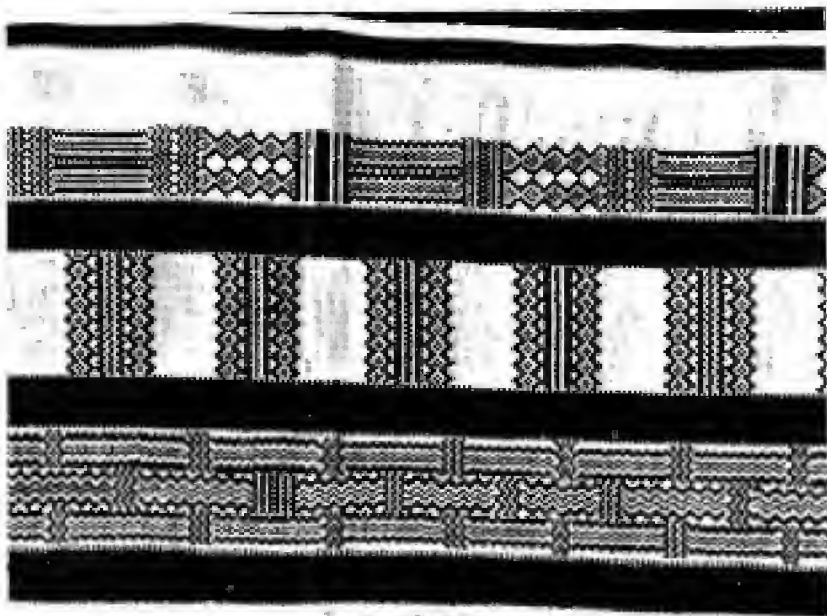


Fig. 13. Detail of embroidery of [top], reverse side

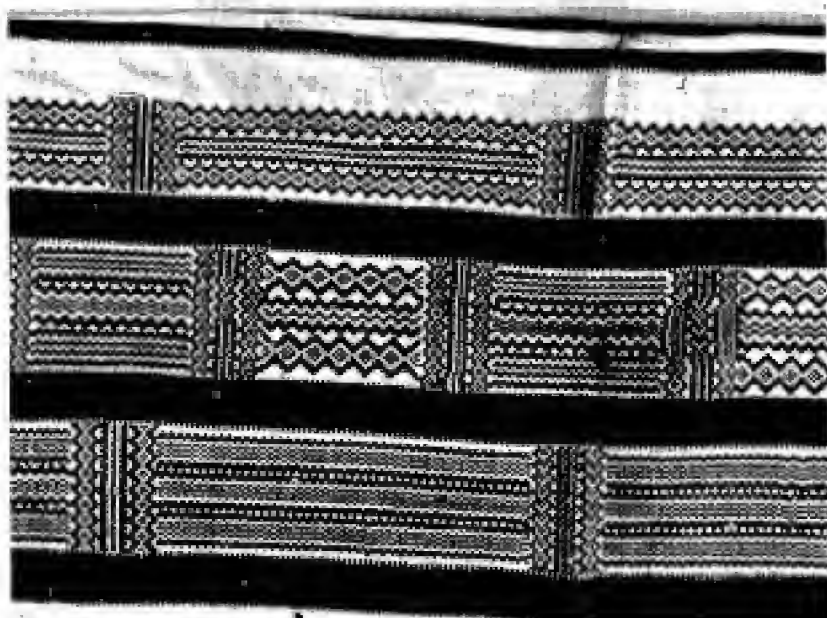


Fig. 14. Detail of embroidery of [top], reverse side

may easily be mistaken for a product of the weaver's loom, and the heavier type has a boldness and character that are wholly admirable. These characteristics and the taste with which they are employed make Toda embroidery a product that will easily stand comparison with the best types of the more familiar work of the European peasant embroiderers.<sup>18</sup>



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<sup>18</sup> This has been fully recognized by the ladies of the Church of England Zenana Mission at Ootacamund (to whom, especially to Miss Stone, who has charge of the work to be mentioned, I am indebted for their mediation in the acquiring of the specimens described in this paper). Much embroidery is now done by the Toda women to be sold through the mission. Some of it is done in the old patterns and colors on khaddar, [torp]-sized for small table-cloths, [pu:taxi]-sized for curtains; a great deal more is done on cloths of white European-type material with small borders made up of simple Toda elements in other colors as well as the traditional ones. One or two new patterns and very slight modifications of the old ones have been introduced into this work and some of these patterns are now finding their way onto Toda garments. Other colors than the traditional ones also are introduced into Toda garments, especially the light blue mentioned in note 4, as well as green. All these foreign elements have been neglected in the paper, though one pattern is of interest in that it has been named, viz. [pu:f] "flower."



# PROHIBITION OF IMPORT OF CERTAIN CHINESE BOOKS AND THE POLICY OF THE EDO GOVERNMENT

SHIO SAKANISHI  
LIBRARY OF CONGRESS

## I

WHEN TOKUGAWA IYASU 徳川家康 (1543-1616) became Shogun in 1603, aware of the advantages in commercial gain and civilization, he adopted a policy of leniency toward Catholic Christianity. Nine years later in 1612, however, because of political intrigues and agitation of the missionaries and their arrogant attitude toward the high officials of the government, he issued a proclamation ordering the complete extermination of Catholics. Their history during the succeeding years was marked by ever increasing persecution and martyrdom. Especially Iemitsu 家光 (1604-1651) who became the third Shogun in 1623, devoted his energy toward the enforcement of the proclamation of 1612, and had such an organ of culture as the Jesuit mission press<sup>1</sup> which flourished in the latter half of the 16th and 17th centuries, destroyed. Even the severest measures, however, were not sufficiently effective to stamp out the new faith, and another proclamation prohibiting the import of all Western and Chinese books that dealt with Christianity was issued in 1639. This proclamation, known as the Edict of Kan'ei 寛永, so named from the Imperial reign, is significant, for, though politically Japan for nine more years adhered to the policy of an open door, culturally and intellectually she closed her door absolutely to foreign intercourse at this time. The purpose of this paper is to trace the course of the development of the Edict of Kan'ei, identify the books that were prohibited, and indicate the method of censorship between the years 1639 and 1730.

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<sup>1</sup> Ryô, Osamu. *Nihon insatsushiki* [History of Japanese printing] (*Seikai insatsu teshi-shi*. Vol. 1). Tokyo, 1936, pp. 207-228. See also: Satow, Ernest. "The Jesuit mission press in Japan," *Transactions of the Asiatic Society of Japan*, v. 27, part 2, Dec. 1889.

## II

The Edict of Kan'ei<sup>\*</sup> prohibited first the import and circulation of thirty-two works by Matteo Ricci and other Europeans, and secondly all those books which aimed at the propagation of Christianity. However, under the Edict, those that only described or mentioned manners and customs of Christendom were permitted entry. Now, the second clause: "All those books whose purpose is to propagate Christianity" is a general statement which requires no explanation. But what were the thirty-two works which were prohibited? Unfortunately no complete contemporary list of them has come down to us, and the various sources which scholars have used in the past show somewhat distressing discrepancies among them. The only dependable sources that we now possess are first, *Kōji koshō* 好事古書 compiled by Kondō Morishige 近藤守重 between the years 1800 and 1820 while he was custodian of books in the Shōgun's court. The second is a manuscript, *Goseikin gomen shaseki yakusho* 御制禁御免書籍譯書 in the city library of Nagasaki. As the title indicates, it consists of three parts: first, a catalogue of 32 titles which were prohibited between 1630-1685 and were not allowed entry even after 1720; second, a catalogue of 18 titles which were once prohibited, but which were permitted after 1720; third, a description of each item in the first two parts. The manuscript bears the date "the fourth month, 1841" and the name "Togawa Harima-no-Kami," Togawa, governor of Harima Province. I identify him as Togawa Yasukiyo 戸川安清 (1785-1868) who was made the governor of Harima in 1816, and for some time was an officer of the Nagasaki Municipal Government. Whether the above mentioned two works were compiled independently or whether Togawa had access to Kondō's work cannot now be determined. However, a careful comparison of these two works with others of similar nature such as the *Samidaru shō* 五月雨抄 by Miura Yasusada 三浦安定 (1725-1789) written around 1784, the *Seisan Kōshin zakkō* 祇齋甲辰雜記 by Mukōyama Gendayū 何山元太夫 written in the early part of the 19th century, and the various catalogues of book dealers in Kyoto and Osaka, discloses that they are far more reliable than the rest, and that of the two, Togawa's is the more trustworthy.

<sup>\*</sup> Kan'ei, Morishige. *Kōji koshō* [Kondō Seisan zenshō. Vol. 3]. Tokyo, 1900, Book 74, p. 215.

In the lists of both Kondô and Togawa, the title *T'ien-hsüeh ch'u-han* stands at the top of a page, and half an inch lower follow the 32 titles of the books prohibited. Now the *T'ien-hsüeh ch'u-han* is a *ts'ung shu* or collection of reprints, which was translated in the latter half of the 16th century, and edited and published by the Roman Catholic missionaries in the 17th century. It is divided into two sections, the first religious and miscellaneous, the second scientific. At first glance, the 32 titles appear to be the contents of the *ts'ung shu*, and some have fallen into this error of confusion, but as a matter of fact only 20\* belong to this series while the other 12 are independent works. For the sake of convenience I have arranged the list in two groups:

A. *T'ien-hsüeh ch'u-han* 天學初函

1. *Hsi-hsüeh fan* 西學凡. [On European Sciences] edited by Jules Aleni and translated by his Chinese colleague. *T'ung Ta-ch'ing-shu* 唐大素寺碑 in the supplement.
2. *Ch'i-jên shih p'ien* 時人十籍. [The Ten Paradoxes] by Matteo Ricci, and consists of a record of ten dialogues. *Hsi-ch'ün ch'ü-i* 西琴曲意. [On Western Music] by the same author in the supplement.
3. *Chiao-yu lun* 交友論. [Essay on Friendship] by Ricci.
4. *Shi-shih-wu yü* 二十五言. [Twenty-five Precepts] by Ricci. Under twenty-five headings, the principles of Christian doctrine are discussed.
5. *T'ien-shu shih-i* 天主實義. [True Meaning of Lord of Heaven] by Ricci.
6. *Pien-hsüeh i-shu* 辨學遺蹟. [Defence of Christianity] by Ricci. A comparative study of Buddhism and Christianity.
7. *Ch'i-k'o* 七克. [Seven Victories] over the Seven Deadly Sins. The author is Diego de Pantoja.
8. *Ling-yen li-hou* 靈言靈旨. [A Treatise on Soul] by Francis Sambiasi and edited by Hsi Kuang-ch'i 徐光啓. He explains the soul or anima under four heads: Substance, Capabilities, Dignity, and Excellence.
9. *Chih-fang wai-chi* 職方外記. [A Concise Geography of the World.] The nucleus of the work was written by Pantoja as an accompaniment to the map of the world brought to China by Ricci. After Pantoja's death Jules Aleni completed it. The frontispiece reproduces Ricci's map of the world.
10. *T'ai-hsi shui-fa* 泰西水法. [Hydraulic Machines of the West] by Sabbatinus de Ursis.

\* In the Chinese sources the last two titles, *Ts'ê-liang fa-i* and *Ts'ê-liang fa-i i-t'ung*, are counted as one, making the total 19.

11. *Han-kai t'ung-hsien tu-shuo* 渾蓋通憲圖說. [The Development of the Celestial Sphere] edited by Li Chih-tao 李之藻.
12. *Chi-ho yüan-pên* 幾何原本. [Principles of Geometry] translated by Ricci and Hsu Kuang-chi.
13. *Piao-tu shuo* 表度說. [Science of Numbers] by Sabbatinus de Ursis.
14. *T'ien-wên t'ieh* 天問略. [An Outline of Astronomy] by Emmanuel Diaz and Li Chih-tao. It is a concise description of the Ptolemaic astronomy and narrates the recent discovery of the telescope and Galileo's observations.
15. *Chien p'ing i-shuo* 簡平儀說. [An Explanation of the Celestial Globe] by Sabbatinus de Ursis.
16. *T'ung-tsu suan chih t'ung-p'ien* 同文算指通編. [Practical arithmetic] by Ricci and Li Chih-tao.
17. *Yüan-jung chiao-i* 圓容較義. [A Short Treatise on Geometry] by the above authors.
18. *Kou-ku i* 勾股義. [A Treatise on the Theory of Rectangle-triangle] by Ricci and Hsu Kuang-chi.
19. *Ts'ü-liang fa-i* 測量法義. [The Theory of Astronomical Measurements] by Ricci.
20. *Ts'ü-liang fa-i i-t'ung* 測量法義異同. [An Analogy between the Old and New Theories of Astronomical Measurements] by Ricci and Hsu Kuang-chi.

B. Independent Works:

21. *Wan-wu chên-yüan* 萬物異源. [True Origin of All Things] by Jules Aleni.
22. *San-shan lun k'ueh-chi* 三山論學記. [Doctrines of Three Mountains] by Jules Aleni. This is an essay on Christian philosophy.
23. *T'ang Ching-chiao pei* 唐景教碑. [The T'ang Nestorian Monument.]<sup>\*</sup>
24. *Ti-tsu ch'ing-chi* 懺罪正記. [Cleansing of sins] by Jules Aleni.
25. *Shih-wei* 十慰. [Ten Consolations] by Alphonse Vagnon.
26. *Chiao-yao chieh-hieh* 教要解略. [An Outline of Christian Doctrine] by Alphonse Vagnon.
27. *Mi-ao chi-i* 彌撒祭義. [Missa] by Jules Aleni.

<sup>\*</sup> Both the *Sansidare-shô* and *Seikai Kôshin sakki* omit this title from their lists, but the reason seems that it was considered to be a supplement to No. 19.

<sup>\*</sup> Kiyôzo Nakamura in the *Edo bunkyo no kiyaku seisaku* suggests that it is not Ricci's work, but that of Emmanuel Diaz, the correct title of which is *T'ang Ching-chiao pei sung-ching ch'uan* 唐景教碑頌正經. See: *Shirô*, v. 11, no. 2, April, 1926, p. 13.

28. *Tai-i p'ien* 代疑編. [Refutation of Buddhistic Misconception of the Transmigration of Soul] by Emmanuel Diaz.
29. *Shêng-chí pai yen* 聖記百言. [One Hundred Teachings of Saint Theresa] by J. Hbo.
30. *Ti-p'ing i chi* 滌平儀記. This work is not listed in the *Sa-sh'u ch'uan-shu t'sung-shu* or other catalogues I have searched.
31. *Huang-i* 況義. [Aesop's Fables] translated by P. Trigault.
32. *T'ien-shu hsü-p'ien* 天主續編. [Lord of Heaven. Second series.]

Since some of the books listed are not available, the translation of the titles in some cases is approximate rather than exact. Nevertheless, we find that out of twenty titles in the *T'ien-hsüeh ch'u-k'an*, only seven are on Christianity; the remaining thirteen are scientific works on such subjects as mathematics, astronomy, and geography. The twelve books of the second group are either doctrinal or Christian apologia. The purpose of the Edict of Kan'ei was to wipe out the new religion, but not to exclude the culture of the West. However, because of the fear and hatred of Matteo Ricci and other missionaries by the officials and also the fact that the teaching of science was often made a cloak for Christian proselytizing, the government made little distinction between the purely evangelical and scientific works.

The policy of Kan'ei was maintained for thirty-five years, and in the meantime there seems to have been no attempt to enlarge the list of prohibited books. On the contrary, according to Kondô,\* when the *T'ien-wên t'ien* by Emmanuel Diaz was brought over in 1630, it was permitted entry. Whether this was only an incident or a continued practice, no record remains, but since the Edict stated that all the Chinese books that dealt only with manners and customs of Christendom were to be allowed, it was not at all strange that an outline of astronomy with a short introduction extolling Christianity should be released from the ban.

### III

In 1685 when a Chinese trading vessel No. 15 came to Nagasaki, there was on it among other books, the *Huan yü ch'ien* 寰有錄 written by F. Furtado and translated into Chinese by Li Chih-tsao. A seemingly innocent treatise on Aristotle's *De caelo et mundo*

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\* *Kôji kasho*, Book 74, p. 210.

had safely passed the hand of an inspector and was auctioned off to a dealer, but soon after Mukai Gensei 向非元成 (1653-1727), a scholar and keeper of the Confucian shrine in Nagasaki which was founded by his father, Mukai Genshō 向井元升 (1609-1677), discovered that the true purpose of the book was the teaching of Christianity. The book was at once seized and burnt. The captain of the vessel lost his permit to visit Japan again, and all the remaining cargo was shipped back to China.

This was the important turning point in the policy of the government. Henceforth very strict enforcement of the Edict of Kan'ei was ordered, and Gensei received high commendation for his keenness and was made the chief inspector of books, which was to be an hereditary office. He proved to be an assiduous officer and prohibited all the books that made mention of even such terms as "Catholic," "Jesus," "Western," "Europe," "Ricci," and "Nestorian Christianity," not to mention books which might have a passage describing the Western hemisphere or a map showing any of the Christian countries. Between the years 1685-1712, the following 16 works were burnt:

1. *Huan yü ts'ien* 寰有錄.
2. *Fu-chien T'ung chí* 福建通志.
3. *Ti wei* 地緯.
4. *T'ien ching kuo wên hou-chi* 天經或問後集.
5. *Ti ching ching-wü lüeh* 帝京景物略.
6. *Hsi-l'ang ch'üan-chi* 西堂全集.
7. *San-ts'ai fa-pi* 三才發秘.
8. *Füan-hsüeh chi* 願學集.
9. *Hsi-hu chí* 西湖志.
10. *Ch'an-chên i-chih* 禪真逸志.
11. *T'an-yü hsia-ho chi* 譯友夏合集.
12. *Fan ch'eng lun* 方程論.
13. *Ming-chia shih kuan* 名家詩觀.
14. *T'an hsüeh chai-chi* 檀雪齋集.
15. *Tsêng-ting kuang-yü chi* 增定廣輿記.
16. *Chien-hu chi* 堅瓠集.

Between 1713-1720, the following 19 titles were either burnt or destroyed:

1. *T'ung-chien Ming-shih ch'uan-tsai* 通鑑明記全載.
2. *Ting-li ch'eng-an* 定例成案.
3. *Hsin li* 新例.
4. *Pên-ch'ao ts'ê-lî lei-p'ien* 本朝淵例類編.
5. *Shan-hai ching kuang-chu* 山海經廣註.
6. *Hsing-li ta-chung* 心理大中.
7. *Yu-hsüeh chi* 有學集.
8. *Hsi-hu chih hou-chi* 西湖志後集.
9. *Su-chou-fu chih* 蘇州府志.
10. *Tan-t'u-hsien chih* 丹徒縣志.
11. *Jen-ho-hsien chih* 仁和縣志.
12. *Ch'ung-shan-hsien chih* 瓊山縣志.
13. *Chin-yün-hsien chih* 縉雲縣志.
14. *Hsin-hsiang-hsien chih* 新鄉縣志.
15. *Chu-lo-hsien chih* 諸羅縣志.
16. *Nan-ch'ang-hsien chih* 南城縣志.
17. *I-yao* 擬曜.
18. *Ming-shih-kun* 明詩藁.
19. *Yen-nien-hsien chih* 延年縣志.

Besides the above mentioned works, *Li-chu shih pien* 理主十編 is given in the *Seisai Kôshin sakki* and *Tokugawa jidai shosetsu ko* 徳川時代書籍考, but it is of somewhat doubtful origin, and I do not wish to include it here. There are also a few other titles which are given in the catalogues of book-dealers of Edo and Osaka as prohibited works. In any case, we are reasonably certain that between 1685-1720, 35 to 36 new titles were prohibited, and when the 32 titles of Kan'ei are added, the total number is either 67 or 68.

It is curious to note that more than one-third of the books thus prohibited in this period belong to the class known as the Gazetteers or the local histories of China. The reason was that they often mention Christian monuments, missions, or missionaries in their texts. In short after the episode of 1685, the attitude of the government toward Chinese books was fanatical and we are often unable to understand its motives.

## IV

Shogun Tokugawa Yoshimune 徳川吉宗 (1684-1751), who came to office in 1716, was an enlightened lover of learning. He was interested especially in the study of astronomy and the calendar system, and with Takebe Katahiro (1664-1739) 建部賢弘, an official scholar of the government, studied not only the Chinese and Japanese but also the Dutch calendar system. Being in doubt as to the accuracy of the Japanese system, Yoshimune questioned a certain Igai 緒飼, who was a pupil of Shibakawa Shunkai 澁川春海 (1639-1716), the astronomer of the government, but to no avail. Yoshimune again consulted Takebe, who also was unable to make any satisfactory reply to his lord's problem, but he recommended his pupil, Nakane Genkei 中根元圭 (1662-1733). Yoshimune was pleased with Genkei and ordered him to translate the *Li-suan ch'üan-shu* 曆算全書 which was compiled by Mei Wen-ting 梅文鼎 (1633-1721) from the Dutch original in 1701. Upon its completion, Genkei told the Shogun that this book on the method of calculating the calendar was only an abridged translation from a Western treatise, and that unless the original could be secured, some of the more difficult problems could not be clarified. At once an order was given to the Magistrate of Nagasaki, and in due course there arrived the original of the *Li-suan ch'üan-shu*, in Chinese translation *Hai-yang li-fa* 西洋曆法 [Western Calendric System], which had been prohibited because of its title "Western," and it was translated by Genkei. It is said that once in the course of an interview, Genkei said to the Shogun: "In Japan because of the prohibition of Christianity, any book that may have a few words on Catholicism or Matthew Ricci is burnt in Nagasaki. We have, therefore, scarcely any book on the study of the calendar. Should you truly wish to have this branch of science studied, I beg of you first to lift the ban on foreign books."<sup>7</sup>

Therefore we are not presuming too much if we consider that the Edict of Kyôbô (1720) by which the ban on scientific books was lifted, issued from Yoshimune's love of learning and Genkei's earnest plea for the cause of science. It reads: "Henceforth any book that deals only with manners and customs of Christendom and

<sup>7</sup> *Yôtokuin go-jikki furoku* [Records of Yôtokuin. Supplement], v. 15 (*Kokushi taishô*; rev. and enlarg. ed. v. 46), p. 292.



does not aim to teach the religion can be imported and sold freely in this country."<sup>8</sup> In spirit this is a mere reversion to the Edict of Kan'ei, but in practice it was a tremendous improvement. During this year, out of the sixty-eight titles prohibited, the following nineteen were permitted:

1. *Chieh-fang wai-chi* 職方外記.
2. *Ts'ê-liang fa-i* 測量法義.
3. *Ts'ê-liang fa-i i-t'ung* 測量法義異同.
4. *Chien p'ing i-shuo* 簡平儀說.
5. *T'ien-wên lüeh* 天文略.
6. *Kou-ku i* 句股義.
7. *Chi-ko yüan-pên* 幾何原本.
8. *Chiao-yu lun* 交友論.
9. *T'ai-hsi shui-fa* 泰西水法.
10. *Hsu-kai t'ung-hsien t'u-shuo* 渾蓋通憲圖說.
11. *Füan-jung chiao-i* 圓容較義.
12. *T'ung-wên suan-chih t'ung-p'ien* 同文算指通編.
13. *Fu-chien t'ung-chih* 福建通志.
14. *T'an-yü hsia-ho chi* 譚友夏合集.
15. *Hsi-t'ang ch'üan-chi* 西堂全集.
16. *Tzeng-t'ing kuang-yü chi* 增定廣輿記.
17. *San-ts'ai fa-pi* 三才發秘.
18. *Chien-hu chi* 堅瓠集.
19. *Hsi-hu chih* 西湖志.

Among the sixty-eight titles of the forbidden books, in addition to the above mentioned nineteen, there are twenty-two others which have nothing to do with Christianity. Whether there had been some specific reason for not lifting the ban from them, is not known. Of the nineteen, *T'ien-wên lüeh* was prohibited again in 1830 on the ground that it had been forbidden before. Kondô remarks that *Chien p'ing i-shuo* and *Piao-tu shuo* as well as a few others were prohibited for the same reason.<sup>9</sup> Perhaps this is another proof of the bigoted attitude of some of the government officials. From 1720, however, the policy of leniency helped much toward the development of medical and other scientific studies.

<sup>8</sup> *Kôji kasho*, Book 74, p. 216.

<sup>9</sup> *Ibid.*, p. 217.

It is interesting to note that the Edo government spared no effort to publish and enforce any act of prohibition, but when once an act was suspended or lifted, the officials made no particular effort to make the fact known to the people. For this reason, although prohibited books were carefully recorded, those which were released later were known only to the officers who were directly connected with the censorship, and the public knew little about them.

## V

In regard to the method of censorship of books, Nagasaki was the only port that had any direct contact with foreign countries, and the system of organized censorship began only in 1685 when Mukai Gensei found the true purpose of the *Huen ya shōkan* to be the teaching of Christianity. Previous to this date, learned priests or Confucian scholars of the city inspected them. After Gensei was appointed an inspector of books, he had four assistants, three recorders and one menial.

When Chinese books were brought to Nagasaki, they were first handed over to the inspector of the ports who recorded the number of packages and transferred the books to the office of the inspector of books. After they were counted again, the content of each was carefully examined and a catalogue made with a general description of the nature of the contents and other useful information which was to be sent to the Edo government. Now in the Shogunate the custodian of books checked and ordered what they wished to have from this catalogue. After their order was filled, all the remaining books were auctioned off to the dealers. Of course prohibited books were confiscated, or destroyed either partially or entirely, or shipped back.

The responsibility of the inspector of books, as Arai Hakuseki 新井白石 (1657-1725) in his *Kottō zaisendan* 骨董雜談 points out, was a serious one. Writing about 1715, he states: "Even today there is an officer in Nagasaki called the inspector of books who examines every book imported and attaches the following statement,—'This book has nothing to do with the teaching of Jesus or his religion. In case it is adjudged differently, I am willing to be punished by the gods of the country.' This is followed by his signature."

The duty of an inspector did not end here, for he was also held responsible for any defect in the book. Chinese books are made up of many small volumes, and sometimes certain volumes were found to be either missing or duplicated. The same thing might happen to pages, or there might be some defect in binding or printing. The inspector's catalogue had to indicate such matters in detail. For suppose the government bought a book and found it to be defective. The inspector had to write to Edo every time asking the government whether he should tender his resignation. Under such a system, it was no wonder that the process of inspection took so much time, and sometimes a ship-load of books was retained in Nagasaki for three years or more.

As to the offender against the prohibition law, when the offence was unintentional, he was released with slight punishment, but when it was due to carelessness or intentional, both the captain and the consigner lost their permits to visit Japan again and the cargo was shipped back, as was the case in 1685. In 1695 when the *T'ien ching ching-wu lich* 帝京景物略<sup>10</sup> met this fate, until the case was settled, all the members of the ship were confined and could not even communicate with other Chinese vessels in the port; the merchants of the city could not supply their needs; the gifts they had presented to a Chinese temple in Nagasaki were ordered to be put away temporarily.

Censored books were usually burnt or certain objectionable passages struck out or pages torn out, and shipped back or sold in Japan. The *Samidare-shō* states: "If it was only description of

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<sup>10</sup> This is a topographical history of Peking. The *Samidare-shō* quotes a memorandum by Iwanaga Gentō 岩永元當 and Katayama Genshō 片山元正, dated the 19th day of the third month, 1695: "The *T'ien ching ching-wu lich*, 28 books in 8 volumes. The volumes 4 and 5 mention Catholic churches and the burial ground of Matteo Ricci. Of the first there are descriptions of the interior of the churches, images of Jesus, life and death of Jesus, and a few architectural details. Of Matteo Ricci it gives an account of his journey to China, how the Lord inspired him to preach the gospel, and his death and burial. The purpose of the book, however, is to describe the manners and customs as well as physical characteristics of the locality, and not to teach the new religion or persuade people to become converts. The author is only struck by the virtuous life of this Westerner. Therefore, this memorandum is prepared to inform and ask your direction on this matter."

manners or customs of Christendom, it was struck out, but if it was doctrinal, it was burnt." Judging from various records, however, I believe this was a general principle, while in practice there was no such uniformity of treatment. For example in 1702 *Ming-chia shih kuan* 名家詩觀 was mutilated and shipped back, but nine years later it was burnt. The *Fu-chien f'ung-chih* 福建通志, which only mentions Catholics in the province, was burnt in 1686, but was released from the ban in 1726.

As far as I can discover there is no instance in the records of any attempt to smuggle in prohibited books, but in the collection of laws and ordinances called the *Wakan kibun* 和漢寄文 dated 1726<sup>11</sup> there is the following passage: "Since there have been numerous intrigues by the Catholics, and since they propagate their teachings and spread their menace, should they attempt to import secretly any books and objects relating to the said religion and be discovered, the persons concerned shall meet heavy penalty according to the laws of the country, and their ships as well as the entire cargo shall be confiscated. Moreover, any one who reports such a case shall be liberally rewarded."

In spite of such rigid laws and zealous enforcement, we find a few instances of prohibited books circulating among scholars. In the Kyôhô era (1716-1736), soon after the government tightened its policy of 1685, one Tsuda 津田, retainer of the Tokugawa clan in Owari Province, obtained by chance a copy of the *Ch'î-jên shih pien* 畸人十篇 by Matteo Ricci, included in the list of prohibited books of 1686. Since he could not make anything of it, he showed it to Ogyû Sorai 荻生徂徠 (1666-1728), the most distinguished Confucian scholar of the time. Sorai was delighted to find something about Christianity, and not only had it copied but also appended a postscript. It reads: "The prohibition of Christianity becomes stricter yearly. Since no book on it is allowed, no one knows about its teaching. It is said that under peaceful disguise it hides venom. It may or may not be true. To leave people so ignorant of any matter, is the fault of the officials."<sup>12</sup> A few

<sup>11</sup> The author Matsunoya Toshiyori 松宮俊仍 (1686-1780), a Confucian scholar, held a minor post in Nagasaki once, and the present manuscript was compiled at that time from the records of the city office.

<sup>12</sup> Quoted by Nakamura, Kiyezo, in his "Edo bakufu no kinsho seisaku . . ." Part 2, *Shûrin*, v. 11, no. 3, July, 1926, p. 428.

years later, Sorai writes again: "Since all the books on Christianity are forbidden, there is no way of knowing what it is like. Still there are many followers. If the Buddhist monks, Shinto priests, and Confucian scholars denounced its teaching, no one would be willing to follow it. Therefore, I wish the government would let the scholars see those books that are in their store-houses and let them judge whether it is a harmful religion or not."<sup>15</sup> I quote these passages as evidence of an intelligent criticism of the policy of the government.

On the other hand, Arai Hakuseki, who was a kind of official moralist to the Shogunate, thoroughly approved of the policy. He writes: "Discussing the fall of the Ming dynasty, a Chinese scholar<sup>16</sup> named the Christian teaching as one of the important factors contributing to it. A very strict proscription of Christianity in our country, then, is not at all our being over cautious. Its enactment, conceived by the far-sighted officials, was extremely timely. To try to offset evil influences of one barbarian with another may, depending on the time and situation, be a good policy."<sup>17</sup> There is, nevertheless, a serious danger of driving wolves out with the aid of tigers."<sup>18</sup>

Coming back to the instances of prohibited books circulating, Hakuseki writes in his *Kottô zatsudan*: "The *Chih-fang wai-chi* 職方外記 by Ricci circulates extensively in manuscript. So do the *T'ung-wên sun-chih t'ung-p'ien* 同文算指通編, *Kou-ku i* 句股義, and *Ts'ê-liang fa-i* 測量法義 as well as a few others among those who study mathematics, but since they are prohibited, they are

<sup>15</sup> *Seidan* 政談, Book 4, Leaf 58.

<sup>16</sup> *Shu Chih-yü* 朱之瑜 (1600-1682) was a Ming official, and repeatedly declined invitations to accept high positions under the Manchus until his loyalty was questioned, and he was forced to flee to Japan in 1645. He served Prince Tokugawa Mitsukuni 德川光圀 (1628-1700) as his teacher and advisor.

<sup>17</sup> The Japanese had seen the Jesuits intriguing to have the Spaniards expelled, the Spaniards pressing for the exclusion of the Dutch, and the Dutch the Portuguese. They also witnessed bitter quarrels between Franciscans and Dominicans. The missionaries hoped that the Edo government might be overthrown and replaced by a party favorable to Christianity, which might well prove to be forerunners of political aggression. The high authorities felt that here was a real danger.

<sup>18</sup> *Seigô kibun* 西洋紀聞, *Arai Hakuseki senshû*, v. 4, p. 790-91.

kept in absolute secret."<sup>22</sup> Ota Kinjō 太田錦城 (1765-1824) in his *Gosō manpitsu shūi* 梧窓漫筆指遺<sup>23</sup> quotes a passage from the *Hsi-hsüeh fan* 西學凡. Mr. Bunshiro Shikada of Osaka has a copy of the *Piao-tu shuo* 表度説, which was in the possession of Lord Mutsudaira Sadanobu 松平定信 (1753-1829).

## VI

To sum up briefly, the Edict of Kan'ei in 1633 forced Japan to close her door to cultural and intellectual contact with the rest of the world. The proclamation of Teikyō in 1685 still further isolated her. A quarter of a century later, however, Yoshimune's love of learning caused the government to adopt a more lenient policy toward scientific works, a policy which was maintained till the latter half of the 19th century. Looking back we see that the prohibition of the importation of foreign books hindered to some extent the development of Japanese science, but perhaps for two centuries it gave to Japan an opportunity for concentration on, and formulation of, her own culture.

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<sup>22</sup> Written probably the year of his death.

## DRAVIDIAN NOTES

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REVIEWING Bloch's *Indo-aryan*, Turner says (*BSOS* 8.210): "It seems unnecessary to follow A. Pillai and L. V. R. Iyer against Caldwell and Gundert and to assume that the final *-am* of Tamil and Telugu nouns is borrowed from Skt. neuter nom. acc. *-am*."

In Arden's *Grammar* (§ 732) we read that the noun-ending *-m(u)* has the variants *-mmu* and *-mbu* in early Telugu. The development of *m* from *mb* is normal in Telugu (*Dravidian Developments* § 28). Evidently *-m* < *-mu* < *-mmu* < *-mbu* is not derived from Aryan *-m*.

In Vinson's *Manual* (§ 22) we read that the noun-ending *-am* might interchange with *-an* in early Tamil. One of the examples given is *maran* = *maram* (tree).

In Kittel's *Grammar* (§ 109) we read that the noun-ending *-am* may become *-an* before a vowel in early Kanara, and that the corresponding accusative-ending may be *-anam*. One of the examples given is *maran* = *maram* (tree), with the accusative *maranam*.

Telugu has *mānu* < *mānu* (tree). According to Winfield, Kui has *maraku* (tree) with the plural *marakka*. These forms show that the *n* of Kanara-Tamil *maran* is older than the separation of the Dravidian tongues. The *k* of Kui *maraku* shows that the ending of Kanara-Tamil *maran* cannot have come from Sanskrit *-am*.

The Kui form *marakka* shows that the *-at* of *maraku* belongs to the singular alone. It is true that Tamil *maram* has the plural *marakka*; but that shows merely that the singular-value of the ending *-am* < *-an* was forgotten in Tamil—just as the singular-value of *tiempos* < *tempus* is forgotten in modern Spanish, which has constructed a new singular *tiempo*.

Corresponding to Brāhmi *urā* (house), in Gōudi we find *rān*, gen. *rōtā*, dat.-acc. *rōtan*, loc. *rōtā*, pl. *rokk*. The form *rān* is an old locative, with the value of *-a* forgotten; there is a locative-ending *-a* in Malta, *-ā* in Kurukh, *-u* in Telugu. It seems likely that the *-u* of Telugu *mānu* and Kui *maraku* is likewise a forgotten locative-ending, and that the early Dravidian form of the word was

\**marah*. Assimilation is seen in Kurukh-Malto *mar* < \**marah*; cp. Telugu *irumu* for \**irumōu* — Tamil *irumpu* (iron). In Kanara-Tamil the ending *-u* may have spread from *maru* to countless other words because it was found to be a convenient singular-ending.

Apparently the Kanara-Tamil noun-ending *-u* came from a locative-ending *-u* or *-uu*, and has nothing to do with Aryan *-u*.

In Bloch's *Indo-aryen* we read (p. 58): "enfin il y a dans les langues modernes une grande série de mots de grande extension débutant par une occlusion cérébrale . . . ici le dravidien qui n'a presque pas de cérébrales initiales ne peut être mis en cause." Initial reverted linguals are found in Brāhui, Gōndi, Kui, Kurukh, Malto, Kanara, Telugu, Tulu; and as a result of synthesis (the not in isolated native words) in Kēlan and Tamil. Bloch's statement would be nearly true if it were restricted to Kēlan and Tamil; but as it stands it is highly unreasonable. What does "presque pas" mean? Evidently Dravidian can be "mis en cause" as an explanation of the Aryan development.

In the *BSOS* 8. 751-62 Schrader undertakes to compare Dravidian with Finnic. He shows that he does not understand Dravidian phonology.

Schrader tells us that Kanara *nānu*, *ānu*, Tamil *jān*, *nān*, Telugu *nānu*, *ānu*, are all from *nān* (1), a form "preserved" in Kēlan alone. He wants us to believe that the *n̄* of *nān* is represented by the initial *n* of *nānu*, *nān*, *nānu*. This is wrong, because he says nothing of the well known history of the forms given. Kanara *ānu* is the older form; modern *nānu* has taken *n* from the plural *nānu*. Tamil *jān* is the older form; modern *nān* has taken *n* from the plural *nān*. Telugu *ānu* is the older form; modern *nānu* has taken *n* from the lost plural \**nām* represented by *manamu*.

With regard to nasals, there is no evidence that the *n̄* of *nān* is ancient. Considered as a palatal, the *n̄* may be compared with the *j* of Tamil *jān*. But Dravidian *j* (consonant-*j*) is not a very ancient sound. Medial *j* is a hiatus-filler or comes from the occlusive *c*; initial *j* is found before *n̄*, and *jā* is regularly derived from an older lengthened *e*. Positive evidence that we should assume *jā* < *ē*, not *jā* < *jā*, is found in Brāhui *ka-* < \**ke-* — Kurukh-Malto *ka-* (die), implying \**ke* or \**ge* as the root of Gōndi-Kui *sā-* < \**cā-* < \**kīā-* < \**kīe-*, Telugu *cā-* < \**kīā-* < \**kīe-*, Kanara *sā-* < \**cā-* < \**kīā-*



< \*kie-, Tamil *ce-* < \*kie- and *cā-* < \*kiā- < \*kie-. The basis of Brāhui *ɛ*, Kurukh-Malto *ən*, Gōndi *ənā* < \*ēnən, Kui *ənu*, Telugu *ənu*, Kanara *ənu*, Tamil *jān* and *-ən*, is evidently *ən*, a lengthened form of the *en* found or implied as the basis of all forms of Dravidian "me" and "my."

Schrader ignores an important point: most of the Kēlan pronouns show developments plainly less conservative than those of its nearest relative, Tamil (*Dravidian Developments* § 54). Kēlan *nān* represents \*jān < \*ēn, with extension of nasality similar to that of Spanish *mancha* < *macula* and Galician *miña* < *mea* < *mea*.

When Schrader takes *nān* to prove that Telugu *nālika* may be related to Hungarian *nyelv* (tongue), he is building without any foundation. And when he compares Tamil *nāvu* with *nyelv*, he does not understand what he is doing. The *v* of *nāvu* is simply a hiatus-filter: *nāv-* < *nā* < *nāk-* < \*nālē < \*nālāk. Furthermore the *n* of *nālika* and *nāvu* is probably not radical; it and the *l* of Kurukh *talnā* seem to be prefixes (*BSOS* 3. 815).

If Finnic is to be compared with Dravidian, we should compare the oldest forms, not later derivatives. Schrader compares Tamil *nāvir* (hair) with Finnish *näsa* (moss). But *nāvir* is derived from *najir* thru \**nāir* and \**nauir*.

When Schrader gives Gōndi *torē* and *torā* [!] as meaning "mouth," he is not altogether correct. Gōndi *torē* means "mouths" and *torā* means "of the mouth." These forms belong to *torōdē*, with *d* changed to *r* before a consonant. Schrader gives the *r*-forms because he wanted something looking like Malto *toro* and Finnish *tursa*. Malto *toro* is cognate with Telugu *nōru*, from a basic \**or* or \**ōr*. The *t* of *toro* is a prefix, added from a preceding demonstrative or from Kolarian *to*.

In various Indic tongues we find a tendency to blend or compound two words into one. In § 170 of Bloch's *Formation de la langue marathe* we read: "*gadhāḍ* est dérivé régulièrement de skr. *gardabha*- et est indépendant de *gāḍhav*." This is a mistake. The regular derivative of *gardabha* is *gāḍhav*; *gadhāḍ* was formed by blending with *ghoḍ* (horse). Likewise Gujarāṭi *gadhāḍo* (ass) was formed by blending with the word *ghoḍo* (horse). Malto has *gadagaro* (ass) beside *goro* (horse). In Brāhui we find *bāmas* (nose) corresponding to *bā* (mouth) and Malto *maso* (nose). In Gōndi we find *mussār* (nose) corresponding to Malto *maso* and a lost Gōndi word \*(s)*ōr* cognate with Telugu *nōru* and Malto *toro*.

(mouth). Schrader is wrong in assuming that Kurukh *zekkã* (hand) developed simply from *kai* thru *\*kajje* with *kã* < *gg* < *jj*. The word is evidently a compound; the second portion alone corresponds to Tamil *kai* (hand), while the first portion represents Kurukh *zedd* (foot).

Schrader compares Finnish *luu* (bone) with Tamil *elumpu* and Kanara *elu*. A Tamil variant is *sapu*: one form represents *\*elump* < *\*elup*, the other *\*ennup* < *\*elup*. The basis *\*elup* is probably a compound like Brähui *bamus*. Kanara *elu* is a reduction of *eluru*, which has the variants *elubu* and *elabu*; all of these forms have lost a nasal and correspond to Tamil *elumpu*. The form that should be compared with Finnish *luu* is therefore *\*el*.

Schrader gives us a "couple of equations" some words meaning "small": Tamil *ciRu* — Hungarian *csiri* and Kanara *kittu* — H. *kicsiny* (*kittik*). This cannot stand. Tamil *ciRu* has a variant given in dictionaries as *ciRRu* (really *ciRR*, used before vowels). Kanara has *kiRu* with the variant given in dictionaries as *kittu* (really *kitt*, used before vowels). Tamil *ciRR* represents *\*kiRR*; cp. *cevi* — Kanara *kivi* (car). Kanara *kitt* represents *\*kiRR*; cp. *mattu* — Tamil *maRRu* (again). The "couple" is thus a single word on the Dravidian side. And Hungarian *kicsiny* is a loan-word from Turkish (*Mém. Soc. franco-ougrienne* 30, 96). An older form of *ciRu* is apparently represented in Sanskrit *kudra*.

Bloch gives Marāthi *dāwā* (left) and Turner gives Nepālī *debre*, *debrē* (left) without any Sanskrit basis. These words came from Dravidian: cp. Tamil *iṭam*, Kanara *eda*, Telugu *edama* and *ṣapaḥi*. Kurukh *debbā* is perhaps from Aryan; likewise Kui *deba*, *debe*: cp. Hindi *debrā*.

I have pointed out the probable Burushaski source of Brähui *luḥ* (horse): *JAO* 55, 355. Another Burushaski word represented in Dravidian is *baṣu* (sult), beside Brähui *bā* (plural *bāk*), Kurukh *bāk*, Malto *bāka*. The Kurukh-Malto *k* is a forgotten plural-ending.

Gōndi regularly has medially and finally *r* for older *r* and *r* for older *ṛ* (voiceless *r*). An apparent exception is to be seen in *maṛ* < *\*maṣir* (daughter) — Kui *maru* < *\*mivan* < *\*mivan* < *\*maṣir*. The Gōndi *r* came from *marri* < *\*mari* < *\*maṛi* < *\*maṣis* (son)

= Kanara *waRi*. The *r* of Brāhmi *masiṛ* (daughter) came from *tr* (sister).

In the *Sitzungsberichte* of the Bavarian Academy for 1897 Geiger discusses the language of the Rodiyas of Ceylon, and lists a number of words belonging to "dem fremden Sprachelement" (p. 25). Among these are *lōddā* (ox), *pālāva* (turtle), *palānava* (elefant), *karuba* (salt), *ilajā* (snake), *rabota* (leaf), *nilāṭu* (water), *uāḍava* (knife). Geiger says that Oppert assured him these words were not Dravidian. I disagree; cp. Tamil *eratu* (ox), *uḷuppaṭakki* (turtle), *paḷ* (tooth), *āvai* (elefant), *uvarppu* (salt-ness), *arā* (snake), *ilai* (leaf), *nē* (water), and Kanara *vāt* (pierce). Another Dravidian-like word is *pella* = Kurakh *pellō* (female). The ending of *rabota* looks like Aryan *patra*.

I have a few more remarks on Bray's vocabulary (cp. *JAOS* 56. 350).

*mālam* (father-in-law): no et. Sanskrit *mātula* (uncle), thru Prākṛit *\*māula*? Ending from *itum* (brother).

*muz* (waist): no et. Tamil *mokkuḷ* (navel).

*narr-* (flee): Telugu *uruk-*. No; perhaps Malto *ner* (snake).

*-ṛṣ* (way, direction): Tamil *-ṛu*. Add Kanara *-hange*, *-hage*, *-henge*, *-hige*, Kui *-hengi*, *-hinge*, *-hingi* (way, manner). Basis *\*həŋk* or *\*hiŋk*, with ancient *h* kept as in Kui *mraḥnu*.

*pāḡu* (thick): Tamil *paṇai*. Perhaps Aryan *\*bhṛḡāu* — Sanskrit *bṛḥu*, Latin *pinguis*, reduced to *\*bhṛḡu* in early Dravidian.

*paṣṣa* (hair): Persian *paš*. Rather Persian *buš* (mane).

*sā* (meat): Tamil *tā*. No; Tamil *a*.

*taf-* (bind, build): Kanara *tag-* (suit), *tagai-* (touch). No; perhaps Kanara *teppa* (raft).

## BRIEF COMMUNICATIONS

### *H. De Genouillac on "Lagash" and "Girsu"*

Lagash and Girsu were practically unknown before M. de Sarzec excavated that group of mounds in Southern Babylonia called Tello. This complex of ruins is roughly oval in form two and one-half miles long by one and one-quarter broad, stretching approximately from northeast to southwest. De Sarzec uncovered some of its hidden treasures intermittently from 1877 to 1900, Captain Cros worked there some months in 1903, and de Genouillac in 1929-31. In the meantime the local Arabs carried on piratical diggings as they wished and brought to light some very valuable material.

It is substantially true (or was so until recently) as King says (*Sumer and Akkad*, p. 98 f.), "Sumerian history may be said to begin" with the revelations made at Tello. The chief results of those expeditions are well-known to cuneiform students. My purpose is simply to consider briefly some of the statements made by de Genouillac in his two volumes, *Fouilles de Telloh*, touching the names Lagash and Girsu: Tome I, entitled *Époques Présargoniques* merely mentions our theme. Tome II deals with *Époque d'Ur III<sup>e</sup> Dynastie et de Larsu*.

In Tome I (p. XI, Note I) the author says: "Lagash (*šir-pur-la*) est le pays, Girsu est la ville, témoin un texte inédit: *Warad-Sin . . . sag-ki-tar gir-su-ki šir-pur-la-ki-a*, "Arad-Sin . . . le contrôleur de Girsu au pays de Lagash." "Au pays" is here based on the translation of *ki* as "country," while it can equally well be read "place, seat."

The author recognizes at the beginning (Tome II, p. 2, notes 2 and 3) some of the difficulties that meet his statement.

On Statue A (I, 5-9) we read: "Gudea, patesi of Lagash, built her (Ninḫursag's) temple in the city Girsu." On Statue F (I, 1-16) we find: "O Gatumdug, mother of Lagash, Gudea, patesi of Lagash . . . in Lagash, her beloved city, a beautiful sanctuary (built)." In other words, both Lagash and Girsu are here called "city." Indeed, the Cones (B and C, X, 22-27) of Urukagina distinguish sharply between the *balḫ* of Lagash and that of Girsu, apparently setting the one over against the other as individual

city units. The same idea seems to be implied in *YBT* (I, No. I): "Arad-Sin . . . chief commander of Girsu and Lagash."

Let me remark right here that we are limiting our problem approximately to the prosperous period of Lagash, about 2300 to 2400 n. c. emphasizing especially that of Gudea and his contemporaries.

De Genouillac says further: "As for Gudea, he seemed to consider Girsu as "le quartier des temples de Lagash" (Cyl. A. 6, 15): "To Girsu the exalted temple of Lagash, mayest thou wend thy steps!"

More than that, he says that within Girsu was the heart of the city, "la ville sainte," "the holy city," of which the primitive centre was "the house of Girsu."

Penetrating a little further into the inscriptions of this period we discover other names of places, almost if not entirely, co-ordinate with Girsu. On Statue G (2. 11-15) we read: "Gudea, patesi of Lagash, from Girsu to Urukugga proclaimed the greetings of peace." "Bannatum built the wall of Urukugga" (*SAK* 23, B, 3, 6-7). Then in the very next lines (7-8) of the same text we read: "he built the city Ninā for the goddess Ninā. Then on the Cones B and C (12, 39-40) we find: "the canal leading to Ninā he (Urukagina) deepened." Ur-Bau says (*Déc. pl.* 8, p. IV f.) "To Inninnu, sacred lady, the great, her temple in Uru I built."

In these quotations we have four places, ministered to by the patesis of Lagash. What relation did they have, if any, to each other?

As long ago as 1838 (*Revue Archéologique*) Amiaud suggested that Lagash was a name that covered a complex of four cities, Girsu, Urukugga, Ninā, and Uru,—each a unit, but all embraced by the name Lagash.

The character and religious significance of these four places may be seen in or implied by some of the following statements. Girsu was the real temple centre of the group. Within its bounds the patesis and kings of Lagash built temples to the following divinities: Ningirsu, the national god, Ninġursag, Nindar, Dumuzi, Gish-tinanna, Ninmar, Nindub, and Meslamtea. In Urukugga ("holy city") they erected a temple to Bau (Babu), Ningiszida, Gatumdug, and Ninsun. In Ninā, there was E-sirara of the goddess Ninā. In Uru, the temple of Inninni (Ishtar) was its sacred possession.

These centres of worship specify the religious pre-eminence of the four named places.

But what were their relative locations and their administrative relations touching Lagash? The excavations at Tello have not yet satisfactorily answered these questions.

Let us now look at some of their equipment given in the texts, without attempting to discuss them individually.

1. "The wall of Girsu he (Urukagina) built" (Cones B and C, 2, 14 F). "The dyke-wall for docking ships in Girsu he (Entemena) built" (YBT, I, No. 1). "The small canal of Girsu he (Urukagina) constructed" (Cones B and C, 12, 30-31). Gudea built in Girsu a seven staged tower, a ziggurat, near Eninnu the temple of Ningirsu (St. G, 1, 5-15).

2. The wall of Urukugga, Eannatum built (SAK, 22, B, 3, 6, 7). "From Girsu to Urukugga he proclaimed the greetings of peace." (St. G, 2, 11-15). "A sheepshearing house in Urukugga he (Urukagina) built" (Cones B and C, 2, 4-6).

3. "For Ninâ, in Ninâ, the city she loves, her E-sirara . . . more lofty than Ekur he (Gudea) built" (SAK 143, H. 2, 2-5).

4. Uru, already mentioned above.

In addition to these citations let us look at some of the passages mentioning the topographical and physical properties of Lagash.

Gudea brought timbers to Lagash from many countries (St. D, 4, 2-14). These he assembled in his city Girsu (Cyl. A, 15, 10). The wall of Lagash he (Ur-Nanshe) built (*Déc. pl.* 2, No. 1); Eannatum makes the same claim.

In the borders (*ki-sur-ru*) of Lagash no litigant [needed] to go to court (St. B, 5, 5-8). Into the plain (*edîn*) of Lagash he (Ush of Umma) came (*Déc. p.* XLIV). The boundary canal of Ningirsu (Lagash's area) he (patesi of Umma) crossed (Nies, *Babyl. Insc.* Part II, No. 1). Ur-Nanshe built the wall of Lagash and made it strong (Tables A-E).

In these sample references Lagash is a plain, is walled, is bounded by canals and has territory. What was it? City or country? Or both?

Before answering these questions, let us look at de Genouillac's dilemma. In RTC (47, 1) he finds: "from Girsu to Lagash" and acknowledges that he cannot explain it. That expression occurs in a text presumably from the reign of Lugal-anda, prior to 2800 B. C. (on one system of dates). May not the difference in dates of the

utterances be the cause of many of the apparent incongruities and inconsistencies in the relations between the so-called four sections of Lagash and between each of them and the general name Lagash?

During the several centuries of the patesis and kings there were doubtless many variations, contractions and expansions in the locations, boundaries, and areas of Lagash and its component parts, so that no fixed boundaries can be laid down for the entire period.

May not the following be a feasible solution of some of the problems touching the contactual, topographical, and national relations of these so-called city-units covered by the name Lagash? As has happened so frequently with many cities down to our own times, Lagash was not simply the country, but a name covering a city complex, composed of parts, of unequal importance, which arose at different periods of development down through the years of its growth as a realm.

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*An Arabic Papyrus Dated A. H. 205*

IN FULL ACCORD with Professor Torrey, I present the following corrected translation, with notes, of the papyrus published by him in the June, 1936, number of this JOURNAL under the title "An Arabic Papyrus Dated 205 A. H."

No attempt is made to exhaust all the possibilities for the names, especially the Coptic ones. I have endeavored to keep the translation as close as possible to that originally given by Professor Torrey.

*Corrected Translation*

This is that which Afīda b. Jarjara al-Zamrudī purchased from Tūsī (2) and Faqrī, the son of Anfar. He bought from them the estate of Anfar Abū Faqrī, (3) which Tūsī and Faqrī had inherited from their father Anfar; and it is the dwelling house (4) which is in the enclosure with certain (other) buildings.

He bought this from them for 3 dinars, (5) in gold coin. The whole amount of the dinars was delivered to Tūsī and Faqrī, (6) and by it (this payment) Afīda became free of obligation to them.

Tūsī and Faqrī, at the time when they sold to Afīda (7) this

estate which they had inherited from Anfar, were of sound mind and body. (8) They sold it with its entrance and its exit, and with what is below and above its surface. If a claim shall be made by anyone (9) in any manner whatsoever or for any reason whatsoever, then upon Tūsi (1) and Faqrī falls its settlement from their own property.

The boundaries of this (11) which Afīda has purchased from Tūsi and Faqrī: The southern boundary is the street; its boundary (12) on the north is the dwelling of Kaisān; its western boundary is the dwelling of al-Fann (?); and its boundary (13) on the east is the dwelling of Yohannas. He bought this from them for 3 dinars in gold coin. (14) (The following witnesses) testified to the acknowledgment of Tūsi, (each) knowing her in person and in name, she being capable of transacting (15) her (business) affairs, knowing what is due to her, and what is required of her: Marqus b. Ishāq (16) and 'Abd al-Šamad, the saddler, wrote his testimony for him with his knowledge and in his presence. (17) And Abūlā b. Hārūn, and 'Abd al-Šamad wrote his testimony for him with his knowledge (18) and in his presence. And Qazmān b. Sayyid Hārūn, and 'Abd al-Šamad wrote his testimony for him (19) with his knowledge and in his presence. And Salm b. Hārūn, and (20) 'Abd al-Šamad wrote his testimony for him, with his knowledge and in his presence. (21) And Ishāq b. Ibrāhīm al-Qarašī, and he wrote his testimony with his (own) hand. (22) And Ibrāhīm b. Ayyūb, and 'Abd al-Šamad wrote his testimony for him with his knowledge and in his presence. (23) And Šairaf b. Farfara (?) and 'Abd al-Šamad wrote his testimony for him with his knowledge and in his presence. (24) ? ? in the *diwān* (?) of Alexandria (?). And this was written in (the month of) Ramaḍān, (25) in the year 205.

#### Notes

*Line 2.* The second word is more likely to be, on paleographic grounds, a *بن* rather than a *بنس*. It is not uncommon in similar documents to have a person's first name only mentioned. A second possibility for the name read as *قفرى* is *قفرى*, *Qafri*, the Coptic *κοῦρις*; cf. Grolmann *Arabic Papyri in the Egyptian Library*, Vol. I, p. 173 (Cairo, 1934).

*Lines 4, 5, and 13.* That the price involved is 3 and not 300 dinars, is assured not only from the word itself, as remarked in



the note, but by comparison with similar documents in which the price rarely, if ever, exceeds a two-figure number.

*Line 7.* The usual formula runs *بصحة من عقلهم وبدلهم*, the familiar "sound in mind and body." The scribe here probably meant to substitute *جسمهم* for *بدلهم*. The four words following run true to the usual formula and are explained through the translation given.

*Line 9.* The first word is *الناس*; the phrase *دعيا من الناس* is frequently met with in similar documents.

*Line 10.* *عاد* is a possible reading, but the usual terminology of similar documents would suggest either *نقاد* or *نقاد*.

*Line 13.* The common Coptic name *يحيى* is suggested instead of *يحيى*; the third and fourth letters are too large for *ya* and *h* respectively.

*Lines 14-15.* These present a problem. The fact to be established at this point of similar contracts is that the witness not only knows the contracting party or parties personally, but that he also knows them to be capable of directing their own (business) affairs. In other words, it is the qualification of the contracting party or parties, and not that of the witness, that is under consideration. The formula usually runs thus:

شهد فلان بن فلان علي اقراو فلان بن فلان يعرفه نفسه واسمه  
... جائر امره الخ

"So-and-so (= X) testified to be the acknowledgement of So-and-so (= Y) whom he (X) knows in person and by name and who (i. e. Y) is in control of his affairs, etc." If this formula is to be followed, lines 14-15 should read:

شهد علي اقرا [ز] توسي ويعرفها لش[ه] واسمها جائر [8] امرها  
تعرف ما لها وما عليها الخ

This would indicate that Tūṣī was the sister of Faqrī, though it does not explain why Faqrī was left out in the formula. It is to be noticed however that Tūṣī is always mentioned first. There may be room then to infer that she was older than her brother; who perhaps was a minor. Then as heirs to the property, they would both have a recognized interest in the sale, and hence be considered as

the "sellers," from whose property future claims must be settled; though the legal responsibility for the sale itself rested on Tusi only.

*Line 16.* For الواج (which is not of the terminology of these contracts) should be substituted السراج, the saddler. Persons are frequently further identified by their trades.

*Line 18.* The name of the witness seems to be قزمان بن سيد شرون.

*Line 19.* The witness' first name is most likely سهم.

*Lines 16, 17, 18, 19-20, 22, and 23.* The three words following *وكتب* in these lines are *عبد الصمد*. We have here six illiterate witnesses who call on 'Abd al-Samad, the saddler, to sign for them, in their presence—a common procedure in like documents. This accounts also for the similarity of the script of these lines as against that of line 31 where Ishāq b. Ibrāhīm signs for himself.

*Line 22.* That the last name of the witness is أبو [ب] is very probable. But it is highly doubtful that we have here the Ibrāhīm b. Abī Ayyub referred to in Professor Torrey's note, since the witness here is illiterate.

*Line 23.* The last word in the line is *يخضر*; for the second last letter is not rounded enough for a *fa*, and there is not enough space between it and the last letter for a medial *ya*. What looks like a medial *ya* is the first stroke (unusually low, it is true) of the final *hā*.

*Line 24.* The village or city in which the transaction takes place is usually mentioned in the first part of similar documents; but perhaps we do have here the exception that proves the rule. There are, however, paleographic difficulties in reading *امكندريه*, both what is read for a *mim* and *dāl* being very doubtful. That the line has both the words مسجد and ديوان seems likewise to be exceptional, for one would hardly expect a public bureau, at that date, to be housed in a mosque and especially in a city as large as Alexandria. The lacunae and the fact that I am working from half-tone reproduction of a photograph do not encourage any further attempts at an alternative reading.

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*Epic iyāt<sup>1</sup> and Blends of Aorist and Optative Forms*

IN HIS interesting paper "The Aorist in Buddhist Hybrid Sanskrit"<sup>2</sup> Prof. Edgerton mentions in section V, § 33, under the heading "Blends of aorist and optative forms (also with future meaning)" certain forms in SP (*Śaddharmapundarikā*) and *Mahāvastu* "which can hardly be other than optatives" used in the sense of past indicatives. For a similar occurrence in *Arđha-Māgadhī* (Amg) he quotes Pischel § 466. It is the object of this note to show that the *Mahābhārata* (Mbh) presents to us some forms of an identical nature, the meaning of which is beyond doubt that of the past indicative or perfect. I shall limit myself here to the root √i- "to go," etc.

Whitney (*Roots*) quotes under √i- the aorist forms *iyāt* and *iyāsa*, but the type is rather doubtful; the form *iyāt* (beside *iyā*) is the normal optative form found in all stages of Sanskrit. We shall see in the following examples discussed below the use of these optative forms in the definite sense of the past indicative.

In Mbh. I. 122. 46-47<sup>3</sup> we read:

rājaputrās tathāivānye sametya bharatarṣabha	
abhi jagmuḥ tato droṇam aśtūrthe dvijasattama	
vṛṣṇayās cāndhakās caiva nūnādcēyās ca pāṛthivāḥ	46
sūtaputrās ca rādheyo guruḥ droṇam iyāt tadā	
spardhamānas tu pāṛthena sūtaputro tyamatṣaṇaḥ	
duryodhanam upāśritya pāṇḍavān atyamanyata	47

Thus we have three verbs *abhi jagmuḥ*, *iyāt*, and *atyamanyata*, respectively in the perfect, optative, and imperfect; the v. l. for *iyāt* as given in Dn<sub>1</sub>, n<sub>2</sub> T<sub>1</sub>: *ayāt* and G<sub>1</sub> *ayācata* are evidently later emendations to get over the difficulty of the optative; for it is difficult to explain the absence of variation in all other Mss. except on the basis that these Mss. treated *iyāt* as an aorist, and therefore did not

<sup>1</sup> I am indebted to Dr. V. S. Sukthankar, the General Editor of the Critical Edition of Mbh for not only placing at my disposal the material which he has collected of the peculiarities of Epic Sanskrit, but also giving access to me to the as yet unpublished part of the *Pana Persea* and the page proofs of the *Udyoga Persea*, for which my sincere thanks are due to him.

<sup>2</sup> JAOS 57. 10-34, and particularly pp. 32-33.

<sup>3</sup> The references to Mbh are to the Critical Edition of the B. O. R. Institute, Poona.

feel the necessity of correcting the optative form by emending it to the perfectly simple imperfect *ayāt*.<sup>4</sup>

In Mbh 5. 19. 10-14 we read :

tasya saṁyam atīvāsīt tasmīn balasamāgame	
prekṣaṇīyantarāḥ rājan suveśam balavat tadā	10
drupadasyāpy abhūt senā nānādeśasamāgataiḥ	
śobhitā puruṣaiḥ śūraiḥ putraiś cāśya mahārathaiḥ	11
tathaiva rājā mātṛyānāth virāṭo vāhinīpatīḥ	
pārvatīyair mahipālaiḥ sukṛteḥ pāṇḍavān <i>īyāt</i>	12
itāś cetaś ca pāṇḍūnām samājagmur mubātmanām	
akṣauhīnyas tu saptaiḥ vividhadhvajaśchakulāḥ	13
tathaiḥ dhārtarāṣṭraśya haṛṣaṁ samabhivardhayau	
bhagadatto mahipālāḥ senām akṣauhīnīm dadau	14

For the form *īyāt* we have the vv. II. T<sub>1</sub> G<sub>1</sub>, 1, 2 *ayāt*, T<sub>2</sub> *gāyu*, G<sub>2</sub> *agāt* (the regular aorist form substituted for √i- according to Pāṇini, but in fact the root aorist of √gā- to go). Here too the optative form is in the midst of imperfect, aorist, and perfect forms (see respectively *āsīt*, *abhūt*, and *dadau*). The past narration is absolutely clear from the tense sequence and context; and it is only the southern version which has emended the text.

Similarly in the narration of a past fight at Mbh 3. 23. 12 we read :

tato 'ham parvatacitāḥ saḥayāḥ sahasrārathīḥ
aprakhyātim <i>īyām</i> rājan sadhvajāḥ paravatais cītaḥ

where *īyām*, though optative in form, is to be construed as a past indicative or aorist, as all the other verbs in this show the same sequence in the tense.

Thus corresponding to the Hybrid Sanskrit forms *apṛśe* (for \**-et*), etc. and Ang. *care*, *paḥape*, *udāhars* and *pucche*, etc. we have seen above that in Mbh *īyāt* and *īyām* we have the Epic use of an optative in the past indicative sense. It is perhaps possible by further study to discover this phenomenon with other roots in the Epic language and other Middle Indo-Aryan languages.

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<sup>4</sup> From the root √yā- to go.

## REVIEWS OF BOOKS

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*The Excavations of Dura-Europos. Preliminary Report of Sixth Season of Work, October, 1932-March, 1933.* Edited by M. I. ROSTOWTZEFF, A. R. BELLINGER, C. HOPKINS, and C. B. WELLS. New Haven: YALE UNIVERSITY PRESS, 1936. Pp. xx + 518, with 54 plates, of which four are colored.

This is the largest "preliminary" report which has yet been published by the joint Dura expedition of Yale and the Académie des Inscriptions. Again the excavators, Professor Clark Hopkins, M. René du Mesnil du Buisson, Messrs. F. E. Brown and H. F. Pearson, Miss Margaret Crosby, win our gratitude for their brilliant discoveries, and the editors deserve equal thanks for their promptness. Like preceding volumes this report is a monument to the energy and organizing powers of Professor Rostovtzeff. It is naturally the Jewish synagogue with its superb frescoes which will attract most general interest, but the sixth campaign would have been outstanding in its importance even without the synagogue. In the following paragraphs we must restrict ourselves to a very few observations on selected fields.

It is a pity that Torrey was not asked to check all the remarks on Semitic personal names before the volume went to press. As the text now stands, this material is defectively treated. Our observations will be restricted to the barest minimum. Graffito No. 617 (pp. 37 f.) seems to contain exclusively unhellenic names, but they are not all Semitic, several being probably Iranian. This is clearest in the case of Μαχ(γ)ουουμας, which has the same ending as Αβραουμας and Αβραουμης (5th century A. D.). The strange Γαδδραου is perhaps a misspelled \*Γαδραους, hypocoristicum for Γαδραθη (\*G<sup>o</sup>dar-Atê, "Atê has decreed," or the like). The name of the latter's father, Σαμμαχαιου, is possibly a hypocoristicum of \*Samm-mal(i)k ("the god Samm is king"; Samm is a common North-Arabian form of Šalm, as shown by Winnett—see the reviewer's observation in *BASOR*, No. 66, p. 31); pronounced Sammakkaî (cf. Sammai, Ingholt, *Berytus*, II, 120); cf. Παχημμαιον in line 6 for R<sup>a</sup>hîmmaî from R<sup>a</sup>hîm-Nansi. The son's name in line 6, Αβεδλαις, stands for 'Ab(i)d-lâhâ, "servant of the god"; it has nothing to

do with Ἀβδάλωνος, 'Ab(i)ḏ-Lāt, "servant of the goddess Lāt." — On p. 133 (cf. 249) we should probably supply τ at the end of Σαθθαδα[ ], reading Σαθθαδα[τ], which most certainly does not mean "Adad made," but is a clear Iranian name, perhaps for \*Sāta-dāta (more recent \*Sāda-dād), i. e., "joy-given," or the like. — The name Σάων (p. 133) is presumably to be equated with a \*Sā'il-lāhā, "the one asked of the god" (for the construction cf. the theophorous names beginning with r'hēm). — The name 'Ubaikan (p. 169) is Arabic, like many other Palmyrene personal names both at Palmyra and at Dura; it is a diminutive of the elative \*'Abhān, perhaps equivalent to classical 'abham, "foreigner, barbarian," etc., by a common dissimilation. — The name Μουρεμος (p. 255) is hardly to be separated from the Μουρημος (M<sup>u</sup>nahēm) of Josephus (*passim*), but there is no need to suppose that the name is Jewish, though it may easily have been. — Σημαθ (p. 256) cannot be connected safely with the goddess Ashima of Hamath, but may well be identical with the Σημαθ of Umm ej-Jimāl, with an ambiguous first element. The curious form Βαργινναχου (p. 293) is evidently a scribal error; one may suggest conflation in writing with Μαλινου. — The comparison on p. 490 with Μαροσαβος, a Christian personal name found in documents of the eighth century, is infelicitous, since this name is naturally taken from that of the famous saint Mār Šāḥā, who flourished in the fifth century. The reading Θημαρσορ for Tēm-Arēš, "client of the god Arēš," is certainly correct.

One of the most interesting single monuments in the book is the bas-relief of the Palmyrene gods Asad and Sa'd (pp. 228-240). Hopkins is right in taking the word 𐤀𐤕𐤕 in the accompanying Palmyrene inscription as meaning "gods," not "gardens," as Torrey suggests. After the discovery by Schlumberger of the still unpublished reliefs of the Jebel eš-Šar', northwest of Palmyra, photographs of which the reviewer was able to examine thanks to Professor Ingholt's courtesy, this is absolutely certain. Both Asad and Sa'd are known Arab deities; for Asad cf. the reviewer's remarks, *JBL* 54, 182, n. 27, and for Sa'd cf. Wellhausen, *Reste arabischen Heidentums*, p. 59 f., and Ryckmans, *Les noms propres sud-sémitiques*, I, 239 f. It is the two gods who are called *g'nāyā*; there is apparently no evidence at all that the slab was broken at the left side of the obverse, so we may read the text as follows: 𐤁𐤕𐤕 𐤕𐤁 𐤏𐤁𐤕𐤕 𐤏𐤁𐤕 𐤏𐤁𐤕 𐤏𐤁𐤕 𐤏𐤁𐤕. However, the inscrip-

tion is hardly complete as it stands. What we have may be rendered, "As a good memorial to the gods Asad and Sa'd (X---) has built the --- houses." A few words about the expression ܣܕܕܐ may be in place. In connection with an unpublished study of the god Genê of Baulbekk (Genêas [conformed in spelling to Gr. γέναιος] Γενάιος, Γενίος; cf. Seyrig, *Syria*, 1929, 335 ff.), the reviewer reached the following conclusion, paralleled at almost every step by suggestions of Nöldeke and Lidzbarski (*ZDMG* 41. 717; *Ephemeris*, II. 82). The passive participle *g'nê*, "hidden," came to be used for the god of Heliopolis, as well as for a class of gods, or perhaps for "god" in general. There are excellent parallels from other countries and periods. The development is further illustrated by lexical reminiscences in the later Aramaic dialects. According to the native Syriac lexicographers (see Payne-Smith, I, 740 b) the feminine plural *genyâ'ê* means "pagan shrines" and "female idols." In the *P'shûtâ* it represents Heb. *'Ātārôt* in several passages, while Ephrem Syrus and Jacob of Serûg use the singular as a synonym of *gaddê*, "(good) fortune, divinity." In Mandaean *ginjâ* means "sacrifice," and, as Gordon has shown recently (*Archiv Orientalni*, 6. 334), ܣܕܕܐ, ܣܕܕܐ means "evil spirit." As Nöldeke and Lidzbarski have recognized, there must be a close connection between the Syriac and Aramaic words and Arabic *jinniyû*, *jinniyatû*, "male, female spirit of beneficent and maleficent type." The phonetic difference is easily explicable as due to morphological adaptation, since Aram. *g'nê* and Arab. *janna* are synonymous.

Nearly a hundred pages are devoted to the Jewish synagogue, dating from 245 A. D. and thus one of the oldest hitherto excavated in any part of the Mediterranean world. The earlier synagogue, probably from the second century A. D., is presumably as old as any yet uncovered in Palestine itself. Mr. Pearson has well discussed the architectural problems that arise, and Professor Kraeling has done yeoman service in interpreting the details of the frescoes. Since the latter have now been repeatedly described and analyzed, the reviewer will refrain from making additional suggestions. The entire subject will, of course, become a favorite theme for iconographic research when the frescoes are published in more elaborate form, with exact drawings of details which remain obscure in the reduced half-tone form of the volume before us.

Among the numerous Greek, Aramaic, and Pahlavi inscriptions which were found in the synagogue, only a few are here published,

and even these are inadequately treated. The reviewer understands that Professor Obermann was only allowed a few days in which to decipher the most important document of all (pp. 389 f.). The Aramaic texts given on pp. 390 and 395 contain bad typographical errors, which are certainly not the fault of the contributors.

Among the important manuscript fragments published on pp. 416-38 is the scrap of Tatian's *Diatessaron* which Kraeling has already published with full discussion elsewhere, and a very interesting Hebrew liturgical fragment from the second or early third century A. D., again with printers' errors, the worst of which is  $\text{ההנה}$  for  $\text{הנהיך}$ . In the Greek documents are many Semitic names, which are unexplained, but which are mainly known from past discoveries. The most interesting Semitic name is *Αραβθαβαλ* (p. 437), which one may hesitantly explain as \**Amat-Ṭāb'el*, "Maid of the god Ṭāb'el." It is true that *Ṭāb'el*, though a common Aramaic personal name, has not hitherto been noted as a divine name.

There is so much intricate coordination necessary in preparing as elaborate a report as the one before us for the press that both care in editing and promptness in publication are probably unattainable, except where there is a special editorial staff, headed by a scholar of competence. The reviewer knows of this happy arrangement only at Chicago, where the Oriental Institute has set a standard of editorial excellence which is virtually unattainable in organizations composed of busy teachers, investigators, and administrators. However, it is a pity that such obvious mistakes as *Ozicha* for *Asicha* and *Oshara* for *Ashara* (p. 435) were allowed to stand. But since we cannot have everything, let us be content with the Lucullan banquet which the joint Franco-American expedition at Dura serves us yearly with exemplary speed!

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1. *A Descriptive Catalogue of the Arabic, Persian and Urdu Manuscripts in the Library of the University of Bombay.* By KHÂN Bahâdur Professor SHAIKH 'ABDUL-KÂDIR-E-SARFARÂZ. Bombay: QAYYIMAH PRESS, 1935. lii + 432 pages. 22s. 6d. net.
2. *Kitâb al-Awraq.* By AL-SÛLÎ. Ed. J. H. DUNNE. London: LUZAC, 1934. xvi + 256 pages.
3. *Tavîkh.* Vol. IX, pt. 1. By IBN-AL-FURÂT. Ed. COSTE K. ZURAYQ. Beirut: AMERICAN PRESS, 1936. x + 243 pages with facsimile.
4. *The Royal Archives of Egypt and the Origins of the Egyptian Expedition to Syria 1831-1841.* By ASAD J. RUSTUM. Beirut: AMERICAN PRESS, 1936. 116 pages.
5. *Ansâb al-Ashraf.* Vol. V. By AL-BALÂDHURÎ. Ed. S. D. F. GOITEIN. Jerusalem: UNIVERSITY PRESS, 1936. 440 + 68 pages, with a volume of annotations.
6. *Histoire des croisades et du royaume franc de Jérusalem.* Vol. II. *Monarchie franque et monarchie musulmane, l'équilibre.* RENÉ GROSSER. Paris: PLON, 1935. 920 pages with maps.
7. *Miscellaneous.*

1. Judging by the recent scholarly output in the Islamic field, Beirut, Cairo, and Bombay remain among the leading cultural centers in the Orient. From Bombay comes this catalogue describing the government collection of one hundred and fourteen volumes housed in the library of the Bombay University and the collection of the University itself comprising sixty-two volumes. The list of the MSS classified according to subjects shows that those dealing with Sufism and poetry are in the majority. The number of rare and valuable MSS in such a comparatively small collection is unusually large. Special mention may be made of the epistles of Ikhwân al-Şafâ' (p. 283), the *Da'wa* of Ibn-Yamûn (p. 32) and *Ta'rikh-e-Alî* (p. 172). The author did not content himself with the customary task of the cataloguer to delineate the internal and external features of the MSS but, whenever possible, added such information as may be of value to the advanced student for research and higher literary criticism. In several places he corrected wrong

references made to works which had been catalogued by other scholars, including Rieu, Nöldeke, Browne, and Nicholson. For instance, the ascription by Ethé and Ivanow of the authorship of the *Nāma-e-Khiyālāt* to the Delhi poet Amīr Khusrāw is proved from internal evidence to be untenable (p. 33). A riddle in an artifice poem (*Qasida-e-Musannaʿa*) by Qiwāmi, to which Browne in his *Literary History of Persia* could offer no "answer," is solved (pp. 15-16). The error of Huart, art. "Bidlīsī" in the *Encyclopaedia of Islam* in mistaking a historical work written in prose for one in poetry is detected (p. 65).

The reviewer has one general criticism to make and it concerns methodology: The descriptive material is unevenly distributed among the various MSS; the colophons, or endings (where incomplete works are concerned), are not reproduced; and the authors' names, which in every instance should have occupied a conspicuous and uniform position, are buried somewhere in places not easy to find.

2. From Cairo comes a fragment of a larger work compiled by the historian and belletrist al-Šūhī († A. D. 946), who was a courtier of three 'Abbāsid caliphs, one of whom, al-Rādi, was his pupil. This fragment is from "Akbbār al-Shuʿarā'" and sketches the lives of a few poets, giving selections from their composition. The editor, a graduate of the School of Oriental Studies in London, prepared the work while teaching in Cairo, using the copy in the Egyptian Library written probably some two centuries after the time of the author. Besides emending the text, and supplementing it with notes from *al-Aghāni*, *al-Ṭabarī*, and other historical and literary sources, and introducing it with ten pages of classical Arabic that give not the faintest suspicion of having been written by a foreigner to the language, Mr. Dunne tries to explain some of the more difficult words. Here a few improvements could be suggested. Add *al-sarī* after *al-sayr* (p. 17, n. 4), for the word defined means "walking at a quick pace." *Dann* does not mean *sawt* (noise, p. 55, n. 3) but "a jar with a tapering bottom." *Al-raḡī* is almost as obscure as *al-ja's* (excrement, p. 55, n. 1), which it is meant to explain.

3. Foremost among the contributions from Beirut is the study of a part of the chronicle of the Egyptian historian ibn-al-Furāt († A. D. 1407). The chronicle begins in Šafar 789 (Feb.-Mar.

1387) and ends in dhu-al-Hijjah 792 (Dec. 1390). It follows the annalistic method, a favorite in Arabic historiography, and emphasizes the events in Mamlūk Egypt. The editor used a photostatic reproduction of a copy in Cairo, which is itself a reproduction of the Vienna MS, considered as ibn-al-Furāt's autograph. He supplied it with historical and grammatical notes. An attempt should have been made to vocalize the many strange proper names, no matter how tremendous the task may have been.

4. Dr. Rustum, a colleague of Zurayq, digs into the Royal Archives of Egypt, studies a number of documents, reproduces some of them, and translates them into English. The documents consist of military and administrative letters, army bulletins, political reports and daily journals of events. The principal signatories are Muḥammad 'Alī Pasha and his son Ibrāhīm, whose times have formed the chief subject of Rustum's studies for years. Around these documents he builds his story placing before the students of contemporary history a provisional estimate of the official view in Cairo on the important events of 1831 to 1833 in Egypt, Syria, and Turkey.

5. Jerusalem, through its young Hebrew University, is beginning to assume greater importance in the field of Arabic and Islamic research. This new era auspiciously ushered in by part of al-Balādhuri's monumental history of which Ahlwardt had issued a fragment as early as 1833. The author († A. D. 893) is one of the earliest and most judicious of Moslem historians. His *Asatib* is a genealogical and biographical record of the leading Arabian tribes and their nobles and an account of the lives of the caliphs and their times. The present volume begins with the life of 'Uthmān ibn-'Affān and ends with the murder of 'Abdullah ibn-al-Zubayr. The MS used for the edition is that of Istanbul, which is based on a copy made in Cairo about a century after the author's death and direct from his autograph.

The editor, Professor Goitein, who has to his credit two other recent contributions in the Arabic field, *Jemenica: Sprichwörter und Redensarten aus Central-Jemen* and *Von den Juden Jemens: Eine Anthologie*, supplied the text with the necessary vowel signs, added an index, table of contents, preface, and introduction in English as well as in Hebrew, and supplemented it with eighty-eight pages of annotations which cite the parallel sources, list

variant readings, and provide elucidatory notes. The type is pleasing and clear, perhaps a little too large for the purpose, the format is excellent, and the paper first class.

A few corrections and suggestions may be made. The reviewer, credited with the English translation of "the whole" of al-Balādhuri's *Futūḥ* (Introduction, p. 9), actually did only a part; the other was done by Murgotten. *Iḥṣāʾ* (Introduction, p. 26) should be spelled *Iḥṣāʾ*. For the sake of consistency, words ending in the *yā'* of the *nisbah*, e. g., *Djāhiliyya* (Introduction, p. 15), should have been rendered *Djāhiliyah*, otherwise *Balādhuri* (same page) should have been written *Balādhuriy*; and words with diphthong, e. g., *Ḥaitham* (p. 14) should have been *Ḥaytham* to conform with *Umayyads* (p. 15). There is no reason why one should resort in English to such awkward forms as *Ḥadjdjādj* (p. 12) when *Ḥajjāj* is just as acceptable. The apparent lack in the University Press of letters with diacritical marks of the required type is a serious blemish, the only one in the entire work. As a result several of the nouns in the Introduction and Annotations are not properly rendered.

6. Among the books recently received from Europe, one of the most important is this history of the Crusades. Vol. I was noticed in this JOURNAL (56. 512). In his second volume Grousset sketches the development of events in Syria and Palestine during the half century beginning about 1131. His material is drawn from William of Tyre, ibn-al-Qalānisi, ibn-al-Athir, and other well-known and previously exploited Arab and Latin chroniclers. He uses Derenbourg's translation of Usāmah (1894) without checking by the more recent English translation. Even the quotations from Derenbourg are not all exact; a comparison of one passage (p. 105, 11. 17-23) reveals two errors of which one is a mutilation of a person's name.

7. Smaller works published in Europe should not escape our notice. One of them is *Das Buch der Alaune und Salze* (Berlin, 1935, pp. 127) by Julius Raska, whose researches in the field of Arabic science are second to no one's. The Arabic text *Kitāb al-Shabāb w-al-Amāl* was widely known in Europe; Roger Bacon referred to it. Raska, who discovered the Arabic original, edits it here critically and concludes that the real author was a Spanish Moslem alchemist who flourished in the eleventh century, about a

hundred years after al-Rāzī, to whom it is popularly ascribed. Another work from Germany is Rudi Paret, *Zur Frauenfrage in der arabisch-islamischen Welt* (Stuttgart, 1934, pp. 70), an interesting study of the problem of woman in Islam which takes cognizance of such modern works on the subject as those of Nazīrah Zayn-al-Dīn.

In French we have *Documents sur la diplomatie musulmane à l'époque du Prophète et des khalifes orthodoxes* (Paris, 1935, pp. 242) by Muḥammad Ḥamīdullāh. The author selected significant documents from al-Balādhuri, ibn-Sa'd, al-Nadīm, al-Mas'ūdi, and other Arab historians, dealing with the origins of the Islamic state, Makkah, al-Madīnah, the Jews, Byzantium, Persia, the Arabian tribes, etc., and translated them accurately. The name of al-Balādhuri's English translator is wrongly given (p. 133). Another French work is *Choix de proverbes, dictons, maximes et pensées de l'Islam* (Paris, 1933, pp. 202) by Edouard Montet, the celebrated orientalist of the University of Geneva. These sayings deal with life in general, morals and religion and are chosen and translated from the Koran, tradition, al-Ghazālī, Sa'dī, Jalāl-al-Dīn al-Rūmī, and other sources.

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*Recherches archéologiques au Col de Khair Khanek près de Kābul.*

Par JOSEPH HACKIN, avec la collaboration de JEAN CARL. Fouilles J. Carl et J. Hackin. (Mémoires archéologiques de la Délégation Française en Afghanistan, Tome VII.) Paris: LES ÉDITIONS D'ART ET D'HISTOIRE, 1936. 38 pages, and 24 plates.

This volume is the report on the excavation of a temple on the eastern slope of the hill of Khair Khanek, some 12 kilometres to the north-west of Kābul. The site was discovered by M. Jean Carl, and M. J. Hackin joined the party engaged in digging; the text of this book is his work.

The main structure, consisting of three sanctuaries, stood on a platform which was found to be the flat roof of an earlier building, in which there were three cellae. The later temple is characterized by the peculiar arrangement of its sanctuaries which are

equal in size: they are placed in a row separated by corridors from the outer walls and the next sanctuary, thus enabling the pious to perform the rite of *pradakṣiṇā* around each sanctuary independently. At the back wall of each cell a sort of solid bank was erected, used as common socle for a group of three statues. Only the feet of a central figure and an almost complete donor, all in white marble, have survived the ravages of time and man. I should like to point out that the figure of the donor shows the same measure, almost baroque style which is well-known to us from thousands of figurines in stucco and a very few stone reliefs from Hadda.

Another piece of sculpture, also in white marble, was rescued from this temple, a group of the sun-god with two attendants. This work was found in one of the gangways, and is extraordinarily well preserved. The group gives M. Hackin an opportunity for a very interesting treatise on the iconography of the sun-god and his associates. The fact that all three of them, even in medieval sculptures of Northern India, wear high boots, and a reference to the "Northern" costume of *Sārya* in early literary sources (*Matsya Purāṇa*) most decidedly hint at a strong Iranian element in the ultimate formation of the sun-god in India. On the other hand, *Sārya* was represented in India proper long before those foreign forms invaded the country: in Bodhi Gayā he is presented in a quadriga, accompanied by two women (second half first century B. C.), and in Bhājā and in the Anantagumphā at Kanḍagiri (first half first century B. C.) similarly in subject-matter, though not in style. In the group of Khair Khaneh the two attendants are male; M. Hackin is certainly right in concluding that there must have been two different conceptions which merge later on, as the females reappear.

It was J. Ph. Vogel who had pointed out that the type of the sun-god seated in his carriage was derived from the representation of Kushāna princes. This applies however to the "*Gestalt*" only, not to its specific religious content. M. Hackin undertakes here to trace the pedigree of the sun-god's male followers, Piṅgala, bearded and with writing utensils in hand, and Daṇḍa, with shield and spear. The learned author maintains that their origin must not be sought in India, but further West, that they must be connected with The Two Holy Brethren, associated by the cults of

Western Asia with the Highest God, or with the Sun, or with the Moon. In following these lines, M. Hackin arrives at a *rapprochement*: Azizos (Morning Star)—Phosphores—Castor—Danda, Menimos (Evening Star)—Hesperos—Pollux—Piṅgala. To overcome the difficulty that Piṅgala wears a beard, the Kabira had to be introduced, Great Gods of Samothrake who were eventually identified "à basse époque" with the Dioscuroi: one of them was bearded, the other smooth-faced. Even this ingenious idea does not, I think, remove all obstacles. M. Hackin himself admits that the attributes of Piṅgala, the writing utensils, cannot be satisfactorily explained by connecting their bearer with one of the Dioscuroi or the Kabira. The author indeed gives a clue to a less circumstantial explanation in his concluding lines: "Le rôle de Piṅgala, le scribe chargé d'inscrire les actions bonnes et mauvaises des humains, ne s'explique et ne se justifie que si l'on admet l'existence d'une divinité psychopompe. Cette divinité psychopompe ne peut être, dans le cas qui nous occupe, que le dieu solaire."

The sun-god is, however, not only a Guide of Souls, but a Judge of Souls, too; otherwise the attributes of Piṅgala would not make any sense. This feature brings the sun-god of Khair Khaneh as close as possible to the Iranian Mithras, who guides the soul of the deceased to the other world, passes judgment on it, and offers it the drink of immortality. In weighing the actions of the dead he is helped by Rashnu and Sraosha. The sun-god's identity with Mithras is put beyond doubt by the coins of Kanishka and Huvisshka, evidence of an overwhelming invasion of Iranian deities into Northwestern India. The difference between the old, Indian, and the new, Iranian, conception of the sun-god is the difference between a personification of a phenomenon of nature and a personification of a moral idea (*sisitliche Idee*). While the rôle of Piṅgala as clerk to the Supreme Judge almost explains itself, the function of Danda is less clear; I incline to see in him the power enforcing Mithras' sentences.

Naturally, these considerations do not account for the exterior of Piṅgala and Danda. Yet I think it not impossible to trace them back to some already known personages, though they, or the prototype of Piṅgala at least, may originally appear with totally different attributes. Anyone a little familiar with the turmoil of religious ideas under Kushāna rule, and their sediment in art and

numismatics, knows how easily and in what a startling way deities changed their attributes and names in those times.

A final remark on the costumes worn by Sūrya and his attendants in Bhūmāra: the coat ending in points to the left and right was known in Northwest India as early as the beginning of the 3rd century A. D., as may be seen on the coins of Kanishka III and Vāsudeva II (cf. JAOS 1936, Pl. preceding p. 429, Nos. 7-8, 12).

This volume, written between two expeditions, is valuable because of the information given of a hitherto unknown type of temple in Afghanistan, and of the learned and stimulating discussion of one of the most intricate problems of Indian iconography.

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*Recherches archéologiques en Asie Centrale (1931).* PAR JOSEPH HACKIN. PARIS: LES ÉDITIONS D'ART ET D'HISTOIRE, 1936. 35 pp. and 26 plates.

The "Expédition Citroën Central-Asie," under Georges-Marie Huardt, was joined by some French scholars, artists, and photographers, and one American, Dr. Maynard Owen Williams, delegated by the National Geographic Society, Washington, in order to make archaeological investigations along the Northern Route of the Tarim Basin, from Kashgar to Turfan. The plan was almost wrecked by the attitude of the governor of Chinese Turkistan, who would not allow any side-stepping from the common route, nor any stay except at previously indicated halting-places. Only after long negotiations were the archaeologists permitted to pay a very short visit to the *ming-ōi* of Kyzil, to the sites of Shorchuk, and those in the environs of Karashahri; ultimately they were permitted to do some little work at Bezeklik and Murtuk.

From the outset the research was planned to complete the work achieved by Alfred Grünwedel, and recorded in his *Altbuddhistische Kultstätten in Chinesisch Turkestan* (Berlin, 1912). The main result, as far as the *ming-ōi* of Bezeklik are concerned, is that Caves 17 and 35 (according to Grünwedel's enumeration) were used and decorated by Manichaeans, previous to a Buddhist occupation. M. Hackin is right in pointing out that this could only have happened after the conversion of the Uigurs to this



creed in 763 A.D. This, however, is only a *terminus post quem*; to judge from the style of the wall-painting in question, in Cave 25, I think it far more probable that it was executed during the second flourishing period of Uigur rule, after Turfan was taken from the Tibetans between 860-873 A.D.

Thanks to his vast experience gathered in Bamiyān, M. Hackin was able to make some observations of great import. One of them refers to the treatment of jewelry in Sanctuary 3 where it is rendered in a slight relief and gilded afterwards; this procedure was commonly thought not to start before the fourteenth century. We have learned now that it was known and in use at least as early as the end of the eighth century, for this date given to Sanctuary 3 and its décor is certainly correct.

The style of the paintings in Sanctuary 3 is Chinese, though rather dry; that of Cave 39 shows a strange admixture of foreign elements. I may add that this peculiar style is already known from the paintings in the "Apsaras-Höhle" at Kumtura, which were always looked upon as very puzzling owing to their strikingly hybrid character. A careful analysis of form will reveal a knowledge of Chinese painting, "Indo-Iranian" influence, and some traits which will eventually, when fully developed, be the characteristics of the Uigur style. Though much cruder, the paintings of the "Nirvāṇa-Höhle," at the same site, belong also to this group. The décor of Sanctuary 3 is certainly earlier in date, and so are the paintings in Cave 37 at Bezeklik which are related to it. All of these pictures are important because they demonstrate the slow transformation of a purely Chinese style into the Uigur style.

The confrontation of two details, identical in content, was an excellent idea for it shows the wide range of quality which must be taken into account when dealing with paintings of the Chinese period in the art of Turfan.

Incidentally, one of the most important paintings published in this small volume is one photographed by the Mission Pelliot at Subāshi: in style it is surprisingly close to the frescoes of the famous "Maler-Höhle" at Kyzil, thus belonging to the earliest stratum of Kuchean, or "Indo-Iranian" art. Unfortunately it does not become clear which Subāshi is meant: the Subāshi given on the map, lying some 45 kilometres east of Turfan, or Subāshi-Lāngār, to the North of Kucha.

Because of the new material and its careful and masterly analysis this book by the learned author is an important contribution, to the study of art in Central Asia.

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*Annual Bibliography of Indian Archaeology for the Year 1932.*

Vol. VII. Leyden: KERN INSTITUTE. Leyden: E. J. BRILL, 1934. Pp. xxi and 175, 10 plates. The same, for the year 1933. 1935 Vol. VIII. xiii and 132 pages, 9 plates. The same, for the year 1934. 1936 Vol. IX. xii and 166 pages, 8 plates.

From the beginning, the *Annual Bibliography of Indian Archaeology*, edited by the staff of the Kern Institute in Leyden, has proved to be of invaluable help not only to scholars of Indian archaeology, Indian history, Indian religion, Indian numismatics, and Indian epigraphy, but also to students with the same interests in Further India, Indonesia, Central Asia, the Near and the Far East. It presents an almost complete record of all the works published during the year indicated which have a bearing on the domains mentioned above, briefly stating their contents, and quoting from the more important reviews and criticisms of them. As the task of compiling the various items and preparing the MS. for press takes some time, there is an interval of two years between the publication of the books and articles concerned, and their appearance in the Bibliography; to cover these two years, the first part of the *Annual Bibliography* brings articles by competent scholars, reporting upon the work in progress or upon new discoveries. These articles are excellently illustrated. Some of them are more than mere accounts, namely important or comprehensive surveys of our present state of knowledge. To this category belongs the first article in volume VII, by Henry Frankfort: "The Indus Civilization and the Near East." There is hardly anyone more fitted than Frankfort to compare the various features of the Indus culture with the corresponding features in Mesopotamia. The discovery of a seal and other objects of Indian make in a well defined stratum at Tell Asmar allows of dating these things, and the civilization they came from, at about 2500 B. C.

A short note by Sir Aurel Stein on his archaeological tours in Southern Persia draws special attention to the finds from Bāmpūr, including a painted ware the décor of which points westwards to Susa I, and eastwards to the Jhalavān Hills in the Indus valley. The article by Joseph Hackin is a short abstract of the same author's *Nouvelles Recherches à Bāmipūr. Mémoires de la Délégation Française en Afghanistan*, Tome IV, Paris 1933, a publication the import of which for the study of early Iranian architecture can scarcely be exaggerated.

"Epigraphical discoveries in India," written by J. Ph. Vogel, reports on the finding of three further versions of Aśoka's Minor Rock Edict, at Yerragudi, Madras Presidency, and on the Gayāmath and Pāḷkigundu hills, near Kopāl, Hyderabad-Deccan; on the synchronism of Khāravēla, of the Hāthīgumphā inscription, with Demetrios, son of Euthydemos, as pointed out by K. P. Jayaswal and R. D. Banerji; finally on an inscription from Mathurā definitely establishing that Huvishka followed Vāsishka in the year 28 of the Kanishka era.

The account on "Archaeological Research in Indo-China" is given by G. Coedès. Mlle. Colani's discovery of a megalithic culture in Upper Laos will prove of great moment for our knowledge of prehistory in Further India and Insulinde. The southward spread to Annam of Chinese civilization in the first centuries A. D. is demonstrated by the tombs of Vinh-yen: extraordinarily well preserved, they give an unexpected good idea of Chinese sepulchral architecture. "Genuine" vaults, not corbelled ones, must have been in general use throughout the whole Chinese *Kut-turkreis* then for they occur also in the graves at Lo-Lang, Korea, and near Port Arthur. The excavations conducted by Victor Goleabew, in 1931 and 1932, around the Phnom Bakheng proved that this Śaiva temple, situated on a wooded hill near the Southern Gate of Angkor Thom, is identical with the "Central Mountain" of the old town of Yaśodharapura, founded by Yaśovarman I towards the close of the ninth century and destroyed by the Chams in 1177 A. D. Angkor Thom, built during the last years of the twelfth century, was the new capital. A very interesting temple, Prasat Ak Yem, dating from the sixth to the eighth centuries, was detected by M. Trouvé. The excellent work done by the Dutch in restoring the temples of Java induced the French to send Henri

Marchal, the Conservator of Angkor, there to study the Dutch methods. After his return, M. Marchal tried his hand at the ruins of Bantéay Srei, old Išvarapura, a complex of temples formerly thought to date from the end of the thirteenth and beginning of the fourteenth century, but actually going back to the tenth century, as demonstrated by G. Coedès. The result of M. Marchal's work is admirable.

In the volume for 1933 attention is called to the excavation of a monastery in Taxila by Sir John Marshall, the largest of its class in the North-West, the ruins of which, besides sculptures yielded a copperplate dated in the year 134. At Nālandā, the famous seat of Buddhist learning, another monastery was laid free, the ninth one, containing seventy-five images of Buddhist and Brahmanical deities in stone and metal.

In his note on Indian numismatics Sir Robert Burn remarks that the question of Azes is far from being settled: N. G. Majumdar brings new material to support the theory of the late Vincent A. Smith that there existed two Azes with Azilises reigning between them; but Herzfeld is of opinion that only one ruler lived who called himself Azes or Azilises.

H. E. Stapleton reports on new finds of Kushān coins and objects of Sūnga facture in Northern Bengal. Until recent times the earliest known historical fact regarding this territory was its incorporation in the Gupta empire between 433-543 A. D. With D. R. Bhandarkar's find of an inscription in Brāhmī characters of Aśoka's rock and pillar edicts, at Māhastan, these new discoveries set the connexion of this region with the rest of India back at least half a millennium.

G. Yazdani, the able and energetic director of H. E. H. the Nizam's Archaeological Survey, relates that the frescoes of the ceilings in the Kailāsa and the Indra Sabhā, at Elūrā, and the wall-paintings of caves IX, X, XII, XVI, XVII, and XIX at Ajantā have been cleared and scientifically preserved. In cave IX a fresco was recovered the motives of which show considerable similarities to the reliefs of the Southern Gate of the Great Stūpa at Sanchī.

J. Ph. Vogel's "Ancient Monuments of Kashmir" is a brief summary of Ram Chanda Kak's book with the same title, interwoven with valuable remarks and suggestions. As to the designs of the tiles which covered the court-yard of the temple at Hārvaṇ,

I think, that, besides an influence from the North-West, a second current from Mathurā of the Kushāna period can be made out.

According to Henri Marchal the Bayon of Angkor, dating from the end of the twelfth and the beginning of the thirteenth century, an epoch notorious for its careless and slovenly building, was restored as well as circumstances permitted. Within the cella of the Central Tower of the Bayon, its vertical shaft was cleared to a depth of 14 meters, the underground water level. The fragments of a very large Buddha seated under a Nāga hood, were rescued from the débris, and successfully pieced together: the statue measures 4.75 m. the socle included. The work at Prasat Ak Yom was continued; a shaft was discovered under the basin in the middle of the cella, filled with débris and leading down to a vaulted chamber of brick masonry, 2.25 m. below the surface of the sanctuary. A slab was found with the Nine Brahmanical Deities, dedicated to Gambhīreśvara, "Lord of the Depth," under which significant name Śiva was worshipped at Prasat Ak Yom, and dated in the Saka year 923 — 1001 A. D.

The most interesting find of the year in Insulinde was the upper half of a bronze Buddha, discovered near the village of Sempaga at the mouth of the Karama river, on the western coast of the Celebes. This work certainly hails from Southern India, for it has all the characteristic traits of an Amarāvati product. I may add that the statue is decidedly earlier than the famous Buddha of Dong-duong. This increases the evidence of the vigorous expansion of South Indian art and culture beyond the seas to the East.

In Volume IX C. L. Fabri reports on the work of the Archaeological Survey of India during the year 1933-1934. Outstanding discoveries were the "Workmen's Quarter" at Harappa, by Pandit Madho Sarup Vats, and highly polished black pottery under a dolmen at Chettipalaiyur, District Coimbatore: the same ceramics are associated with neolithic implements in Baluchistan and Southern Iran. Then the new rules regulating the grant of licenses for archaeological excavation within protected areas are reprinted; they will prove of great value for learned societies or institutions intending to make use of this innovation.

A statue of a Bodhisattva from Mathurā was "rediscovered" in the Allahabad Museum by K. G. Goswami. The figure has lost its head; the pedestal bears an inscription to the effect that the

statue was set up in the second year of Mahārāja Kanishka by the nun Buddha[nītrā]; the work is therefore one year older than the well-known Bodhisattva in the Sarnath Museum, dated in the year 3, and set up by the monk Bala. The head of an Avalokiteśvara, reproduced on Pl. IV/a, is of higher stylistic than iconographic moment, being one of the very rare specimens which show the transition from Kushāna to Gupta style. 150 Brahmi inscriptions were found in Ceylon in 1933-1934, most of them short ones, but of considerable historical interest since they verify the statements of other sources. The excavation of the Kaṭṭaka Chetiya at Mihintale raises again the question of stūpa building in Ceylon, of its similarities and dissimilarities with contemporary structures of the same kind on the mainland. In Further India, the accidental find of a huge piece of Cham sculpture near Binh-dinh led to further excavations, especially of the Tháp-mâm, "The Demolished Tower," situated about 300 meters to the north of the ancient northern rampart of Chaban. Chaban is the Vijaya of old inscriptions, the capital and main citadel of the kingdom of Champa from the eleventh to the fifteenth century. It was learned that the Chams had profited by the thirty years' occupation of their capital by the Khmers in the beginning of the thirteenth century, evidently they became acquainted then with the use of laterite for the construction of basements.

Baron Robert von Heine-Geldern contributed an extensive survey of "Prehistoric Research in Indonesia." The author is one of the outstanding scholars in the field of ethnography and prehistory of Further India and Indonesia.

As all these instructive notes, reports, and papers, appear in addition to the proper contents, the complete bibliography of the year in question, these volumes of the Annual Bibliography are, like the previously published ones, an almost inexhaustible mine of information. They are really indispensable, and I cannot imagine any scholar or library going on without them.

LUDWIG BACHROFFER

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*Barabudur.* By PAUL MUS, with a preface by M. GEORGE COZBÈS.  
Hanoi: IMPRIMERIE D'EXTRÊME-ORIENT, 1933. Pp. \*302  
+ 802.

This is a book as monumental as the subject with which it deals: it embraces, indeed, far more than the title implies, extending to a detailed analysis of the entire history of Buddhist art; it is as much a treatise on Buddhism itself as on Buddhist art in particular, and must be considered as no less epoch making than the earlier work of Foucher, which forms in a general way the point of departure from which the author proceeds. The sections of the work, apart from the *Avant-propos* of 302 pp, which is reprinted from recent nos. of the *RÉFEO*, are entitled: Introduction; 1. Les interprétations architecturales; 2. Interprétations religieuses; 3. Le symbolisme du Barabudur; 4. Barabudur et le problème des cinq directions; 5. La valeur cosmique du stupa; Appendice, Les Sept Pas du Buddha et la doctrine des Terres Pures; all of the foregoing making up Tome I. Also, 6. Genèse de la Bouddhologie Mahāyāniste; forming the whole of Tome II, part 1, and presumably still to be followed by another part.

The particular characteristics of the work are its constant correlation of texts and monuments; and its emphasis on the continuity of the pre-Buddhist and Buddhist traditions, suggested by the motto printed on the wrapper, "Le Brahmanisme des Brāhmana . . . père du Bouddhisme," taken from Sylvain Lévi, with the significant omission of Sylvain Lévi's continuation, "qu'il lui a légué une regrettable hérédité."

For M. Foucher, the earliest Buddha images are Gandharan.<sup>1</sup> However this may have been, their iconography, as he also points out, is Indian, and only their style is Hellenistic. The image pre-existed verbally (and therefore as a mental image) already in the old lists of the 33 major and 80 minor *lakṣaṇas*, on which (so far as they can be represented in art) the authenticity and efficacy of the image is held to depend throughout the subsequent history of Buddhist art, which is nothing if not "correct."<sup>2</sup> The Indian

<sup>1</sup>See, however, the recent important discussion of the dates of Gandharan sculptures by Benjamin Rowland in the *Art Bulletin*, vol. XVIII, 1936, pp. 387-406, where it is concluded that all can be situated between about 200 and 500 A. D.

<sup>2</sup>To say, as does Mr. Codrington (*Legacy of India*, p. 98) that "Indian

Buddha type on the other hand (which prevailed after a short struggle in the second and third centuries) has behind it not only the same iconographic prescriptions (in effect a *dharma* mantra), but also for the seated figure a tradition going back to the Indus Valley culture and for the standing figure a tradition going back to the Maurya stone figures of Yakṣas, and beyond these no doubt to wooden forms.

The Brahmanical cult had been aniconic, and this condition persisted in early Buddhism so far as the Buddha himself is concerned, who is represented by symbols only from the moment of the Nativity and not merely after the Awakening; it is no more, then, a matter of distinction between Bodhisattva and Buddha than it was of an inability of the artists to represent the human figure; the situation is similar to that of early Christianity, where symbols are employed by evident choice. We must not deduce from our own anthropocentric point of view and artistic preoccupations that the development of an image represented an advance. "on entendait recourir à une forme non pas inférieure, mais supérieure de l'œuvre d'art en substituant des symboles à l'image" (p. \*66). The intellectual and abstract symbol corresponds to a contemplation; the image to the will, and prayer. Actually, however, the image is not a portrait, but retains a purely symbolic quality, and remaining a symbol is still a support of contemplation (*dhya*śāmbhā): it is rather to the Bodhisattvas and later Tārās that prayer is addressed. The whole tradition is based on the presumption of two distinct orders of existence, that of this and that of another world, between which a communication is only possible by a mediation; the incantation (*brahman*), for example, is outwardly what the deity (Brahman), knowable only thus by analogy, is inwardly. It is precisely in this sense that the Buddha says that "He who sees the Word (*dhammam*) sees Me." The audible doctrine, the physical body, and the work of art are all alike from this point of view "factitious" bodies (*nirmāṇa-kāya*), to be valued in their significance rather than in their actuality; it is in this sense that the image functions as a substitute, and implies a real presence.

The Vedic rite involves a simultaneous edification and reintegration of the divided deity and the sacrificer himself. The sacrifice

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iconography is a medieval ascription, derived from a folk idiom, eventually crystallising out in literary form" is a verbatim contradiction of the plain facts.



is literally a de-votion, the sacrificer being both built into the altar by virtue of the modulus taken from his own person ("On façonne donc ainsi sa personne dans la bâtisse, où le dieu et l'homme s'unissent enfin," p. 254), and by a symbolic self-immolation on the altar itself. It is M. Mus' particular merit to have brought out clearly that the basic principles of the Brahmanical rite survive in the Buddhist cult, as also, of course, in the corresponding Hindu iconolatry. The patron, who takes the place of the sacrificer, is incorporated into the image in the same way by means of a modulus taken from his own person, and also when an image is made of gold, by using an amount of gold equal to his own weight, and by which he is therefore represented metaphysically, "gold" having throughout the transcendental values of spirit, light, immortality. And finally, the act of worship is only perfected to the extent that an assimilation of the worshipper to the transcendental form of the Buddha is effected. Just as the first operation of the artist in *ākhyāna* had involved a self-identification with the imitable form of the idea to be represented, so is it for the spectator guided by the visible representation to effect a similar *adequatio rei et intellectus*: all this, supported by means of citations from the Chinese inscriptions recorded by Chavannes, and other sources, is in perfect accord with the formulated mediaeval Indian aesthetic, which makes of the determinants (*vibhāra*) the occasion of the "finding of a common ground" (*mādhārāya*), and identifies the tasting of the "flavor" (*rasa*) of the work with a supra-sensual intuition.

In this connection M. Mus speaks almost always, with what seems to us less than his usual penetration, of a ritual "magic": magic, however, is an application of, and not to be confused with, metaphysics or even religion; the Vedic rite is metaphysical. To speak of "the magical essence of the man" (p. 205) is ontologically meaningless; the only possible "essence" that can be distinguished from an "existence" is a spiritual or metaphysical principle, the idea or reason of the thing,—an immaterial entity, then, far removed from any plane affectible by any kind of magical operation. In using such a term as "magic" one must preserve the senses in which the word is employed in the environment considered; it must be remembered that from an Indian point of view, "magic" is not merely a supernatural power, but an operative, however lowly, science.

It is surprising, too, that M. Mus should repeat that the specifically Buddhist position is a denial of the *ātman* (p. 128). It has been sufficiently shown by Mrs. Rhys Davids and others that such well known expressions as *na me so attā* state in fact that "This (body and soul) are not my *Ātman*"<sup>2</sup> and not at all that "There is no *Ātman*." If a *pudgala*, or individuality is later introduced, as a designation of the bearer of the burden of retribution, this is not the very man, but the body and soul that are not the *Ātman*; *pudgala* and *attā* together correspond to the "two selves" of the man discussed in the Jaininīya Brāhmana, I. 17. 1 (see JAOS 19. 2. 116). The fact is that the transcendent person of the Buddha is the *Ātman*, and if there is a Buddhist, as there is also a Brahmanical, *nirvāṇa*, this nirvāṇic state of desperation has nothing to do with the expression *anattā*, which merely denies to the physical and psychic elements of the empirical individuality any spiritual character.

Mahāyāna and Hinayāna are not so much successive as complementary aspects of Buddhism; the former related to the latter somewhat as are the Apocryphal to the canonical Christian "Acts"; in both cases the apocryphal material dominates the cult and iconography, and one may say that for a knowledge of Buddhism as a whole the art is no less important than the literature. It should not be overlooked that "apocryphal" by no means necessarily means "spurious," but rather "esoteric." The monastic type of the Buddha is common ground alike to Mahāyāna and Hinayāna Buddhism; but as M. Mus has formerly shown in an important discussion of "Le Buddha paré" (BÉFEO, 1928) there is also a royal type in which the values of the Buddhas not only as ascetic but also as Cakravartin and in the Sambhogakāya are represented; the Messiah is both king and priest. M. Mus has done more than anyone else to bring out clearly the distinction between the esoteric doctrine taught by the Buddha to an audience of Bodhisattvas in the fifteenth chapter of the "Lotus," from that which had previously been received by the ordinary monks; it is not a matter of the "closed" or "open" hand, on the contrary all is

<sup>2</sup> As St. Paul expresses it, "The word of God . . . extends even unto the sundering of soul from spirit" (Heb. 4, 12). The distinction is that of the "knower of the field" from the "field" itself, Bhagavad Gītā, Ch. XIII.

always offered, but it depends upon individual capacity what can be received. The royal Buddha images of the Pala period and of South-eastern Asia are an embodiment in stone of the earlier cult practice of offering royal adornments to the Buddha, and placing them on the image, of which practice we learn both from Sinhalese records and from Hsüan Tsang.

We are particularly interested by the fact that M. Mus interprets *sambhoga* (p. 659), not as a "joint fruition," but as a "perfect, or complete, fruition"; just as *sambodhi* is not a joint awakening, but a perfect awakening. We have reached an identical conclusion by a quite different route, which may be briefly outlined here. In the R̥gveda the R̥bhus are said to have made Tvastṛ's single bowl into four, by which we understand an extension of the four directions. We are familiar with the idea of a derivation of nourishment from the four quarters. Now in the well-known miracle of the four bowls, he makes into one the four feeding bowls that are brought to him by the Regents of the Quarters, and eats from this bowl alone. That is the *sambhoga*, a total fruition without necessity of local motion; just as *sambuddhi* is "total awakening." The whole cycle represented by the work of the R̥bhus and converse "miracle" corresponds to the dictum "I being one, become many; and being many, become one" (*Saṃyutta Nikāya*, II. 212).

It would be impossible even to summarize within the limits of a review all that M. Mus has to say, much of it novel and profound, on the nature of the stupa. It is by no means merely a funerary and memorial monument, but veritably an icon, animated either by relics, by a written word, or by the mere fact that the Word has been spoken on the site where the stupa is erected, or however otherwise the necessary connection has been made between the here and now and eternal yonder. The principle underlying the use of relics is that which is reflected in the magical practice of establishing a connection with, and obtaining a power over, an absent or unknown person by means of its traces, such as parts of the body, clothing, personal possessions, or footprints. The cult of the Footprints, moreover, is evidently on the one hand a continuation of the old Vedic motif of the tracking of the Hidden Light by means of its spoor (*padā*), and no less evidently on the other hand analogous to the Christian doctrine of the *vestigium pedis*.

The stupa is an imitation of the cosmic body of the transcendent Buddha, just as the fire altar had been an imitation of the cosmic

body of Prajāpati; a habitation in the same sense that the Christian church has been regarded as simultaneously the tomb and the house of God. Every stupa is literally an hypostasis of the universe; the vertical axis which is the principle of its whole design is the axis of the universe; the stupa is the universe in a likeness, rather than a likeness of the universe, just as the image is the Buddha in a likeness and not a likeness of the Buddha. The Indian stupa or temple is moreover not merely a constructed space but also a constructed time, an image of the revolving year. The perambulatory processions of deities, or kings (in cars which are themselves imitations of the cosmic archetype which is the Vedic chariot of light (*jyotiratha*) and analogous to the Biblical chariot of fire) are imitations of the solar perambulation of the universe: in the case of the immovable stupa or temple this temporal motion is indicated, if not always by the actual wheels of the "ratha" and draught horses as at Kōṣṭhik, then by repetitions of the central architectural form on a reduced scale, visible in succession to the perambulant spectator (p. 383); or in the same way when a stupa is provided with eyes or images on all four sides, these are the oriented aspects of the single person whose true position is axial. The stupa is not so much erected on a given site as upon a diagram of the orient, a *rose des vents*, a lotus that is in principle the whole extent of space (p. 381; Maitri Up. VI. 3); the ground plan of the stupa, notably at Baraṇasur itself, is a *maṇḍala*.

The central axis is the Vedic *śkambha*, and embodies the whole extension of the building in principle. This axis is a *staves* that holds apart heaven and earth; it is then synonymous with the thunderbolt, lance, or other weapon with which the chthonic serpent is transfixcd in the beginning. To this day a fixation of the site, by which earthquakes are avoided, is effected by a symbolic transfixion of the head of the chthonic serpent, Anantu (an excellent reference can be added here, to the Legend of the Iron Pillar at Delhi, printed at the close of Grierson and Waterman's *Lay of Aṅga*). On the other hand, the axis, passing upwards through the dome of the stupa becomes a mast (*yasti*, *yūpa*); it is the path of the vertical *asṛi* or vector by which the Spirit ascends at death from the apex of the "heart" to pass out through the dome of the cranium; the mast above the stupa is the same thing as the flame emerging from the *ūṣṇīṣa* in many Buddha types. In this connection M. Mus discusses the "bow and arrow" symbol on early

Indian coins, in which M. Foucher thought that he recognized a stupa in cross section. We do not question the analogy of the vertical pillar with its armed point to the royal lance around which a stupa is built by the Sinhalese king Dutthagāmani (cf. the lance symbol on punch-marked coins),<sup>4</sup> but we think M. Mus is mistaken in excluding at the same time the idea of the bow and arrow, and in seeing in this idea only a late interpretation or mere allegory, "especially as the bow and arrow play so small a part in Buddhist imagery" (p. 118). This last is scarcely a matter of fact; it is for example as an expert archer that the Buddha wins the hand of Yaśodharā (as Rāma that of Sītā), and notably in the Asadisa Jātaka that the Bodhisattva is represented as an "unfailing archer" (*akkhapa-vedhā*) in a miraculous legend that is unmistakably a symbolic statement of the *sātrātman* doctrine. We think that the bow and arrow symbol, while not necessarily the symbol exclusively of a stupa, is such in a certain sense; the ultimate significance of the symbol is most clearly stated in the Mupdaka Up. II, 2, 2-4, where it is a question of making of oneself the arrow, and of a penetration of the sun; the position of the sun in the vault of heaven, of the "eye" of the vault of a dome, socket of the mast in the dome of the stupa, and that of the foramen of the skull are all analogous, and all considered in Indian texts from the same point of view, that of the ascent of the spirit of the deceased. We should say that while the bow and arrow symbol does not represent a stupa directly, its idea is one that is also embodied in the symbolism of the domed stupa.

Another point discussed at some length is the virtual identification of the Buddha with Brahmā. M. Mus remarks that the Buddha "est le père du monde pour le sauver, non pour l'avoir créé" (p. 624). This is a false antithesis. Creation and redemption are inseparably linked. There can be no escape from the necessity of birth except for those who have been born. The Way that leads beyond the world passes *through* the world. The setting in motion of the Wheel of Principles (*dhamma-cakka*) is at the same time a creation and a predication: it is in a similar sense that the Sun in Maitri Up. VI, 30 is called at once the cause of

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<sup>4</sup> In a Champa (Annam) inscription of 658 A. D. we read "It was there that Kaundinya, the greatest of Brāhmaṇas, planted the javelin," i. e. established his sovereignty.

creation, heaven, and of final emancipation (*sarga, svarga, ap-sarga*). The lamentation of the Devas when the Buddha hesitates to set in motion the Wheel of the Law (as Agni hesitates in RV. X. 51 to accept the position of driver of the cosmic chariot) shows what values still really inhere in the Buddhist symbol, however its meaning may have been restricted in a special context to mean a "first preaching" at Benares.

All that we have been able to do is to suggest the wealth of the documented material to be found in Barabudur, and to emphasize the establishment of a method in which the researcher no longer envisages Buddhism as an isolated and revolutionary phenomenon, but in its setting, and no longer considers the texts alone or the art alone, but each as an alternative expression of the same underlying theses. This amounts to a veritable bringing to life of the "dry bones" of what might otherwise have been a merely statistical archeology.

We may remark that criticisms of M. Mus' work by Przysluski have already appeared in the first volume of the JHAS. and in JISOA. vol. IV.

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*A propos des voyages aventureux de Fernand Mendez Pinto. Notes de A. J. H. CHANIGNON, recueillies et complétées par Mlle. M. MÉDARD. Pékin: IMPRIMERIE DES LAZARISTES, 1936. Pp. xviii, 417.*

Fernão Mendez Pinto is one of those disturbing writers with whom one can never quite dispense and whom one can never quite believe. His *Peregrinação*, published in 1614, more than fifty years after Pinto's return from the East, reads like an interesting tale of adventures, which, however, inspires little confidence. In spite of this feeling of mistrust, shared by most scholars who have studied him, certain episodes, for which he is the sole authority, have found more or less credence, since they seemed to supply some sort of information where no other was available. Obviously such uncertainty as to the value of a historical source is extremely unsatisfactory and a thorough-going study of all its statements would be a real service to scholarship.

This task was undertaken by Mr. Charignon, a retired railway engineer with a taste for scholarly research who lived in Peiping for many years and published an edition of Marco Polo in three volumes.<sup>1</sup>

Dying in 1930 he had not yet completed his task which was continued by his collaborator and sister-in-law, Mlle. Médard. Inspired by loyalty to the deceased, the latter has now edited his notes, adding her own comments into which, unfortunately, has crept a modicum of animosity against Professor Pelliot who, in the above-mentioned review and in an obituary notice on Charignon, in *T'oung Pao*, XXVII, pp. 457-458, had ventured to express dissent with various views expressed by this scholar. Charignon modestly called himself an "interloper in science" (p. 65) and Pelliot had expressed his regret that he, working in isolation, was doomed to sterility in many of his efforts (*T'oung Pao*, XXVII, p. 458). Mlle. Médard has now set herself up as Charignon's champion and has rendered him the very doubtful service of turning Pinto's vindication into a vindication of Charignon.

The result of all this, I regret to say, has been an unsatisfactory book. Not content with presenting Charignon's views on Pinto, which were staggering enough, Mlle. Médard, in a long introduction, reopens the discussion on a number of problems of historical geography, many of which were considered settled by competent scholars. A single example will show how far Mlle. Médard is carried away by her zeal.

The island of Java has been identified with the term *Shê-p'o* 蘭婆 by Groeneveldt, Pelliot, Hirth and Rockhill, and others for the T'ang and Sung periods, being first called *Chao-wa* 爪哇 in the Mongol period. Now in this book not only is the former identification denied, *Shê-p'o* being placed in Indo-china, but even the identity of Java with *Chao-wa* (which either in this form or in its corruption *Kua-wa* 瓜哇 is still the current name for this island) is called in dispute, and in a special appendix (pp. 377-384) an attempt is made to prove that Kublai Khan's expedition against *Chao-wa* in 1293 was not directed against the island of Java but against Cochin-china.<sup>2</sup> One can scarcely believe one's

<sup>1</sup> *Le Livre de Marco Polo*, Pékin, 1924, 1926, 1928; cf. *T'oung Pao*, XXV, pp. 150-169.

<sup>2</sup> Cf. also A. J. H. Charignon, *Le Livre de Marco Polo*, III, pp. 153 ff.



eyes in reading such a wild assertion. Are Charignon and Mlle. Médard not aware that there is independent testimony regarding this expedition in two Javanese historical works; the *Nāgarakṛtāgama* and the *Pararaton*, both very brief, it is true, but nevertheless unmistakable!<sup>2</sup> Do they not know that a number of geographical and personal names in the Chinese account have been identified with certainty with Javanese names? The identifications which the authors suggest instead, such as *Vodave* for *Ma-jé-pa-ksieh* 麻喏巴歇 (Mojopahit, the famous capital in Java) are absolutely arbitrary and fantastic.

Coming to the discussion of Pinto's text, one would expect that the authors should have taken notice of critical work done by others. Now in 1926 Father G. Schurhammer, S. J., published an excellent critical analysis of Pinto's text.<sup>3</sup> It is rather disconcerting to find that the authors' knowledge of this very thorough study is confined to certain quotations from it in H. Bernard's *Aux Portes de la Chine, Les Missionnaires du seizième siècle* (Tientsin, 1933) and that ignorance of the German language prevented them from reading it (p. 98); it is worse that they, not having read this article, nevertheless feel at liberty to suggest (p. 301) that Schurhammer's criticism is motivated by Jesuit spite against Pinto, who at one period of his life entered the Jesuit order, leaving it again shortly afterwards. A careful perusal of Schurhammer's analysis might have saved the authors from certain bold assertions. Pinto, as is well known, has a great number of names which so far have baffled all attempts at identification, but none of these seem to present any obstacles to their ingenuity.

It is certainly quite startling to find that they make Pinto sail up the Amur river, ascend the Sungari and somehow emerge in the Gulf of Liaotang (pp. 146 ff.). The fabulous "island of *Calempuy*" they identify with *Pailin Pu-lo* 巴林部落, the territory of the Barin tribe in Inner Mongolia on the Silamuren (pp. 69, 164) and in the royal tombs which Pinto and his companions looted they recognize the tombs of the K'i-tan emperors, discovered some years ago (pp. 164 ff.).<sup>4</sup> They also attempt to trace Pinto's route on his

<sup>2</sup> Cf. N. J. Krom, *Hindoe-Javaansche Geschiedenis* (1931), pp. 355-356.

<sup>3</sup> *Asia Major*, III, pp. 71-103 and 194-267.

<sup>4</sup> Cf. e. g. Jos Mullie, "Les sépultures de K'ing des Liao," *Young Pao*, XXX, pp. 1-25.



pretended travels in the interior of China proper, following him into Säu-ch'uan and Yünnan (pp. 290 ff.).

The arguments on which all this is based are unfortunately extremely fragile. The port of *Buxipalem* is explained by the Chinese words *P'u-hsi-pei-lu* 浦西北路 which is translated as: "route vers le nord, port à l'ouest" (p. 147), a translation which is untenable in any case, even if it were permissible to seek the identification of a place-name with an arbitrary string of Chinese monosyllables. The river *Sumhepadano* is *Su-mo pu tung-nan* 粟末部東南, "la rivière qui sort au sud-est des Soumo," the *Sumo pu* being the tribes that live along the Sumo, i. e. Sungari (p. 156). The mouth of the river where the adventurers were at anchor, is called *Patebenan*, which is explained as *p'a-ti-pei-nan* 臥地北南 "se traîner du nord au sud," because going from the mouth of the Amur to the source of the Sungari one "drags oneself from the north to the south" (p. 157)! The name of the region of *Tenquilem*, ten miles from *Calemptuy*, is supposed to represent the words *T'ung-ch'ing lu* 東京路 "route vers la capitale orientale" (p. 184).

Really, in this way it is possible to explain everything. Not one of these identifications has the slightest value and nobody but an entirely uncritical reader will be led astray by them.

If that part of the book which deals with "Calemptuy" and Pinto's supposed travels in the interior of China must be pushed aside as utterly unconvincing, some consideration may be given to the discussion of the identity of certain place-names along the south coast. Here again the authors would have profited by taking notice of earlier studies, such as T. T. Chang's work on *Sino-Portuguese Trade from 1514 to 1644* (Leyden, 1933) and Pelliot's review of this book in *T'oung Pao*, XXXI, pp. 58-94, entitled "Un ouvrage sur les premiers temps de Macao." Our authors devote much attention to the identification of such places as *Liam-po*, and *Chin-cheo*. The first of these, generally taken as Ning-po, they explain as a wrong reading for Tien-po 電白 on the coast of Kuangtung (pp. 135, 370-371), where there was a centre for trade up to 1536. For *Chin-cheo*, which generally is identified either with Ch'üan-chou or with Chang-chou, both in the neighbourhood of Amoy, Mlle. Médard wishes to distinguish between three different places all called by that name: (1) *Ch'üung-chow* 瓊州, the capital of Hainan (p. 117),

(2) the island of *Ch'ing-chow* 青州, a little to the south of Tien-po, on the coast of Kuangtung (pp. 371, 373), and (3) one belonging to Macao (p. 373).

One hardly knows what to say about all this. Chinese names being what they are, one can nearly always find some sort of resemblance with any given name and so make them fit the context. But the method is wrong. Are we to suppose that Pinto's nomenclature is entirely different from that of other early writers? Only by a careful comparison of all the sources is it possible to arrive at satisfactory conclusions. If there is anything at all to be retained from the authors' identifications, they will have to stand this test. But then the identification Liampo—Tienpo immediately falls to the ground, apart from other reasons, by the fact that Gaspar da Cruz, who was at Canton in 1556, says that in 1548 the Chinese fleet, destined to attack the Portuguese at *Liampo*, could not go up *so far north* and therefore limited its operations against the establishment of Chin-cheo;\* as to the latter place there may be doubt in some cases whether Ch'uan-chou or Chang-chou is meant, but Mlle. Médard's attempt to distinguish between three different *Chin-cheo's* is certainly hard to reconcile with the numerous sources which mention this name; the question is too complicated to be discussed in this review.

As an example of the inadequate identifications of other words than place-names I finally must quote the one given on p. 105, where is said that "les capitaines des jonques qui étoient dans le port s'assemblèrent en conseil qu'ils appellent *bichara*"; this word is explained as *pi-shang liang* 秘商量 "délibérer en secret," whereas it simply is the ordinary Malay (Hindustāni from Sanskrit *vicāra*) word for "to reflect upon, to discuss, to meet in council."

The circumstances in which this book appears make it particularly unpleasant to be ungracious about it. Yet scholarship would be badly served by allowing this book, which has all the trappings of scholarship, to pass as the last word on its subject. Pinto's book has already caused too much confusion to permit it to get worse confounded by this kind of commentary. Mlle. Médard has shown a love for this type of research, besides great ingenuity; if she will only break through the isolation which Pelliot regretted in the studies pursued by Charignon, she may, no doubt, yet be able to produce

\* Cf. *T'oung Pao*, XXXI, p. 86.

results that are more convincing than most of those presented in this book.

The illustrations and maps in the book are interesting and it has a good index.

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*The Birth of China.* By H. G. CREEL. New York: REYNAL and HITCHCOCK, 1937. 402 pages, xv plates.

This is professedly a popular book, and as such it should be very successful. It is, moreover, a book that will interest sinologists, even though at times it will irritate them. For the sinologist, the book will be of value for the first hand information Dr. Creel presents of the recent excavations at An-yang and Hsün-hsien, for his opinions on the oracle-bone inscriptions, and for the summaries of work on bronze inscriptions. The general cultured reader will find the book readable and interesting, and not too technical. Dr. Creel is to be praised for writing an account of ancient China that is primarily a cultural history based on recent discoveries in archeology.

But Chinese archeology is only beginning, and a book like this will be in part out of date before it can be printed. The Chinese themselves are taking such projects seriously at last, and it is probable that some time will elapse before our ideas of ancient China can be stabilized in the flood of new information that should soon come pouring in. Already the excavations at An-yang since 1934 have forced us to revise our ideas of ancient Chinese culture.

Under the circumstances it seems unnecessary to examine the book closely for errors, but a few general suggestions may be made. Dr. Creel is still a little too self-conscious, using the word *I* too often, and still likes to coin new names. The *Document Classic* may be more accurate than the *Book of History*, but it hardly seems necessary to change a name with which everyone is familiar. Nor is it necessary to give the titles of the classics in Chinese. Where Dr. Creel is dealing with the inscriptions or the archeological finds, he is excellent. But in those chapters where he makes use of historical material, he should have used more the work of European sinologists like Maspero, Granet, and Karlgren. He does not ap-

pear to have special training in either archeology or ethnology, and while this may be of advantage in writing a popular book, it is a drawback from the point of view of the specialist. He is also given to advancing theories at times without sufficient evidence, as in his account of the development of the term *T'ien*. He treats of the duty of avenging a father, without apparently being aware of the monograph of Haenisch on that subject (*Die Rachepflicht . . . Deut. Morgen. Gesell.*, X, No. 1-2, 1931). His treatment of social organization would be improved by a closer acquaintance with the discussions of ethnologists on this subject. His notes, as long as he was going to have any, should be much fuller. In short, while he is familiar with the field work that has been done so far, with the Shang writing, and with the bronze inscriptions, he should have consulted more western authorities when he stepped beyond the field where he can speak at first hand.

But in spite of these defects, the book is stimulating and valuable. The plates are good, there is an index, a very limited bibliography, and an introduction by C. W. Bishop.

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*Omei Illustrated Guide Book.* By D. L. PHELPS. Harvard-Yenching Institute Series. Chengtu, China: WEST CHINA UNION UNIVERSITY, 1936. 354 pages, profusely illustrated.

There are four mountains in China particularly sacred to Buddhists, and each is devoted to a particular Bodhisattava. Mt. Omei is located in the western province of Szechuan, and is sacred to P'u-hsien (Sanskrit, Samantabhadra). There are no critical studies of these mountain cults, of the sort that Chavannes made of Tai-shan. P'u-t'u is well-known to tourists from Shanghai, and R. F. Johnston has described Chin-hua, but there has not been even an adequate description of Omei, although it has been visited by many westerners.

Dr. Phelps, the professor of English Literature at the West China Union University, is to be thanked for this volume containing a Chinese text and illustrations, with an English translation. It was not only published in Szechuan, but its format and binding are Chinese. It is really a guide to the mountain and its temples, a Chinese Baedeker for visitors. There are a number

of prefaces, including one by Dr. Phelps, and translations of poems. Unfortunately there is little historical material, and no description of cults. Dr. Phelps has not made a critical study, but he has given considerable material that would be necessary to anyone making such a study. He has done a very creditable piece of work within its limitations, and it is to be regretted that more of the men with such opportunities do not take advantage of them.

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*An Annotated Bibliography of Selected Chinese Reference Works.*

Compiled by Ssu-YU TENG and KNIGHT BIGGERSTAFF. Peiping: YENCHING UNIVERSITY, 1936. 271 pages.

Previous to the publication of this volume, the only work in English in this field was Wylie's *Notes on Chinese Literature*, published in 1867. The resemblance between the two books is not close enough to justify a detailed comparison, and it is enough to say that the more recent work is very much more valuable. It is a selective of Chinese works, and therefore there would be little point in criticizing it for omissions, since no two scholars would agree on exactly which works should be included. No European titles are given. Within these limitations the work appears to have been very carefully done, and should be of great assistance to scholars. While research would require more information, the book covers an enormous field, and would make an admirable starting point for the scholar who does not know exactly where to look for the information he requires.

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*Territoires et Populations des Confins du Yunnan.* Translated by J. SIGURET. Peiping: VETON, 1937. 307 pages, 4 maps.

The Chinese volume which M. Siguret has translated indicates a new attitude on the part of intelligent Chinese toward the non-Chinese peoples of southwestern China. It is a series of monographs on the aboriginal groups of the province of Yunnan, written by six different Chinese scholars of the younger generation, published by the Bureau of Popular Education of the province, with the approval of General Lung Yun, the governor of Yunnan, who contributes a preface. One suspects that the attitude of the

USSR toward similar groups in Russia and Siberia has been the inspiration for these studies. The attitude toward the various peoples is sympathetic, which is quite a change from the attitude of former generations of Chinese. The studies are carefully made, chiefly from first-hand information, but the interest is political rather than scientific. The point of view is the present situation and what to do about it, and the studies are intended as the basis for definite policies. The nationalistic attitude may be gathered from a sentence which appears on a map over the territory of Burma: "Territories nous appartenant autrefois et tombées entre les mains des Anglais."

At times the observations are a little naïve. "La race tibétaine a des ressemblances avec les Américains et les Européens. Dans leur caractère et leurs coutumes ils ont des points communs avec les gens d'Amérique et d'Europe. De plus, le principe de leur écriture est le même: la transcription phonétique" (p. 33).

Chinese characters for proper names are given, and M. Siguret has provided a few notes. The maps are good, and there is an index. The format is excellent. The bibliography contains only thirteen titles, the first of which is L. H. Morgan's *Ancient Society*. The large amount of work done by both Europeans and Chinese on groups like the Lolo is ignored, although the translator refers to Dr. Rock and others in the notes. While the *Nan-chao Yeh-shih* is used, other Chinese historical works, like the *Man shu*, are not mentioned. Nevertheless, the volume contains a large amount of material which will be valuable to the ethnologist, and is a creditable achievement. M. Siguret deserves thanks for his careful translation.

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*Introduction to Literary Chinese.* By J. J. BRANDT. Peiping: VITCH, 1936. 352 pages.

The excellence of this book, first published in 1927, is shown by the fact that it has been necessary to issue a second edition. It is the best introduction to literary Chinese in English at the present time, in spite of the fact that much more work remains to be done by grammarians before we shall have a thoroughly satisfactory textbook. Brandt is especially to be praised for his treatment of the particles.

There seems to be no change in the material from the first edition, and even the preface is practically the same. The chief changes are that the number of pages has been reduced from 503 to 352 by changes in the format; that the table of contents is now in the front of the book; and that in the indices the references are to pages and not to lessons—a very welcome change. The original publisher was the North China Union Language School, but the second edition is published by Henri Vetch, who has made many improvements in the format.

*La Philosophie Morale de Wang Yang-ming.* By WANG TCH'ANG-TONG, S. J. Shanghai: T'OU-SH-WE PRESS; Paris: GEUTHNER, 1936. 217 plus 31 pages.

This is Vol. 63 of the series *Variétés Sinologiques*, and increases the debt of Sinology to the Jesuits. There has been no satisfactory treatment of Wang Yang-ming in any European language. Henke's study (*The Philosophy of Wang Yang-ming*, Chicago, 1916, and "A Study in the Life and Philosophy of Wang Yang-ming," JNCBRAS, 1913, pp. 46-64) is based on incomplete texts, and is unsatisfactory in other respects. P. Wang's study is based on the complete works of Wang Yang-ming, on Huang Tsung-hsi's *Studies of the Doctrines of the Ming Philosophers*, the Ming dynastic history, and six recent studies of the thought of Wang Yang-ming by Chinese scholars.

The first chapter gives an account of the period, and of Wang's life, and the remaining seven are studies of his doctrine. Five chapters are devoted to the development of the idea of *liang-chih* 良知, which P. Wang wisely prefers to romanize, rather than translate. He points out how dangerous it is to translate Chinese technical terms into western philosophic language, and illustrates the point by the use of such terms as "intuition" and "pragmatic" when applied to Wang.

There is a chronological table of Wang's life, an admirable study of technical terms giving Chinese characters, an index of proper names, and an appendix of thirty-one pages giving the Chinese text of important passages in Wang's works. The reviewer questions whether it is accurate to say that Wang Yang-

ming exercised a preponderating influence on Chinese thought during the 16th and 17th centuries, for the school of Chu Hsi was always strong. But there can be no question of his importance both in China and Japan, and the present study is an important contribution to our knowledge of a great thinker.

JOHN K. SHRYOCK

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*The Crucial Years of Early Anglo-Chinese Relations, 1750-1800.*  
By EARL H. PRITCHARD. *Research Studies of the State College of Washington*, Vol. IV, Nos. 3-4. Pullman, Washington: STATE COLLEGE OF WASHINGTON, 1936. Pp. 95-442.

This volume is a product of mature scholarship and forms a valuable contribution to the field of modern Far Eastern history. Essentially it is a study of the background of the Macartney embassy of 1793. The first chapters deal successively with the establishment of the Canton system (1751-61), the progress of the East India Company's commerce (1760-80), private, foreign, and country trade and the struggle for control of the China trade as a whole (1764-1800). The five following chapters then analyze the origin and history of the abortive Cathcart embassy (1787-88) and of the Macartney embassy which succeeded it. A dozen statistical tables form the appendix. Twenty-eight pages of critical bibliography provide the most complete summary extant of the published and unpublished materials relating to the subject in western languages.

Dr. Pritchard's interest in this subject is of long standing. He first completed a master's thesis at Illinois, which was published under the title "Anglo-Chinese Relations during the Seventeenth and Eighteenth Centuries" (*University of Illinois Studies in the Social Sciences*, Vol. XVII Nos. 1-2, March-June, 1930, preface dated March 1930, pp. 244). In contrast to the present work, however, it may be noted in passing that this earlier volume suffered from a number of handicaps: in surveying two centuries in 175 pages, the author was obliged to touch upon a number of questions, like cultural contacts, the origin of Chinese anti-foreignism, and early missionary activities, with which he was not able to deal in more than cursory fashion. Working in this coun-



try, he was obliged to rely entirely on published materials, with the result that the latter part of his text was often little more than a rearrangement of data from Dr. H. B. Morse's *Chronicles of the East Indian Company trading to China (1635-1834)* (5 vols., Oxford 1926-29). The text was a laborious compilation, overstocked with facts, for which the footnotes gave a plethora of references on each point. In short the volume bore the stigmata of a thesis. While the chief India Office materials were noted in the bibliography, there had been no opportunity to make use of them.

The work now published is a decided and gratifying contrast to this early thesis. While in England as a Rhodes Scholar at Oriel College, Oxford, 1930-1933, Dr. Pritchard had opportunity to comb various series of records in the India Office. On these materials, plus the Macartney papers in the Wason collection at Cornell, his present work is chiefly based. Other archives and libraries in England, France, and the United States were also searched. The resulting manuscript was presented as a thesis for the D. Phil. (Oxon.) in 1938 and was later revised.

While Dr. Pritchard has gone over many of the materials summarized in Dr. Morse's monumental *Chronicles*, the result has not been duplication. As Dr. Morse was himself the first to say, the *Chronicles* are primarily a factual condensation, designed to serve as a basis for further work in an almost untouched field; of the five volumes, only two (vols. II and V) bear on the period 1750-1800, and their main focus is the trade rather than the embassies. Dr. Pritchard has used manuscripts which were not surveyed for the *Chronicles*. His monograph in fact exemplifies the type of special study which Dr. Morse, before his death in 1933, foresaw and encouraged.

To the historian of international trade, Dr. Pritchard's statistical tabulations should prove invaluable. In chapters II, III, and IV, vast masses of data from the archives have been refined into graphs and pithy summaries, the methods of conducting and financing various aspects of the trade have been neatly analyzed, and, at least to one not a statistician, the result is imposing.

For the student of international relations an equally interesting story is unfolded. Its high point is the Commutation Act of 1784, regarded by Dr. Pritchard as "the most important event in the history of Anglo-Chinese relations prior to the abolition of the

Company's monopoly . . . in 1833" (p. 146). In brief, by lowering the English tea duties this measure struck a deathblow at the enormous Continental smuggling trade into England; together with the wars of the succeeding years, it helped the English East India Company to establish its domination over the China trade; this facilitated a great increase in Sino-British trade, from 1784 on, and this in turn made the old Canton system more and more inadequate and moved the British eventually, in view of difficulties both in England and in China, to consider the sending of an embassy.

In the light of later events it is interesting to note how very early the British representatives on the spot in China had seen what was needed to facilitate commercial expansion. As early as 1754 Mr. Frederick Pigon had proposed an embassy to Peking: "The Ambassador must come in the King's name, but in a Company's Ship, he must never have been in China before; at least not in the Company's Service, or belonging to any Ship, he must be a Man of some rank, or figure, an Officer in the Army would do; he should be a Man of understanding and probity, and not too haughty . . ." (p. 125). Such an embassy, continued Mr. Pigon, should request permission for the Company both to trade and store goods at all the ports of China, and to maintain a permanent resident at Peking,—objectives which were later sought in the manner he suggested, but which were not realized for over a century.

While certain highlights in Dr. Pritchard's account may thus be indicated, its fascinating intricacies can hardly be touched upon here. His hundred and fifty pages on the embassies of 1787-93 appear exhaustive, within the limits of the materials in English, and put the Macartney embassy in an entirely new setting. Too often it has been assumed that this embassy was a protest (against the restrictions of the Canton system) on the part of the East India Company. The present volume makes it plain that, far from being desired by the E. I. C., whose position at Canton was comparatively prosperous, the embassy in reality represented the new forces of the Industrial Revolution and was sponsored by Henry Dundas in order to open new markets for new products,—rather than to preserve or assist the fortunes of John Company.

This subject deserves more extended investigation. And indeed, as might be expected in so new a field, Dr. Pritchard's work par-

ticularly in his earlier chapters, has the value of raising many problems for further research: what more can be said of the proposals in 1784-85 to finance the expanded British trade by sending Indian opium to China? (p. 316-17). What can we learn further of the relations between the E. I. C. and the British country trade, both in India and at Canton? What were the internal economy and politics of the Cohong? Questions such as these indicate the magnitude of the field to which this volume, together with the *Chronicles*, will now serve as a *vade mecum*.

It goes without saying that the Chinese side of the story remains to be told. Dr. Pritchard lists over a score of Chinese sources in his bibliography, however, and uses in his text a number of documents, drawn chiefly from the *Shêng Hsün* 聖訓 of Ch'ien Lung, the *Kuo-Ch'ao Jen-yüan chi* 國朝柔遠記 and *Yüeh Hai-kuan chi* 粵海關志, most of them relating to the embassy of 1793. From these sources there emerges no real support for the contention that Macartney performed the *kotow*.

In the absence of Chinese characters anywhere in the volume, romanization naturally becomes important and errors in it are of more consequence. Some of these are noted below. The sinological reader should be reminded, however, that Dr. Pritchard's work stands squarely upon the western materials, from which it derives its great value, and that many of his citations of Chinese sources are made for bibliographical purposes with an eye to their future rather than their present use. For example, his citing of "*Shih Liao Hsün K'an* ch. 10 memorials dated Ch'ien Lung 20-5-11, 20-5-16. . . ." (p. 128) does not give the Chinese dates converted into their intelligible western equivalents, presumably because the absence of pagination in the *Shih Liao Hsün K'an* 史料旬刊 makes the Chinese dates the best means of locating the material, which is cited as of a purely supplementary nature. How far the further use of Chinese sources will revise the author's picture of affairs in China remains to be seen. At least he may be pardoned for digging through only one mountain at a time.

Facts and figures based on manuscript sources can hardly be checked by a reviewer. Dr. Morse's *Chronicles* are for some reason cited as "*Morse Chronicle*" throughout. Magniac is misspelled four times as Magnac (p. 179). There are a good many faulty romanizations. *Hsing Pu* (p. 210) should be *Hsing Pu* 刑部.

Yang Ting-chang (p. 130 note 25) should be Yang Ting-chang 楊廷璋. *Shih Liao Ts'ung K'an Ch'u P'ien* (p. 425) should read . . . *Pien* 史料叢刊初編. Chang Ku Ch'ung Pien (p. 424 and throughout the volume) should read *Chang Ku Ts'ung Pien* 掌故叢編. Similarly Wen Hsien Ch'ung Pien (p. 425) should read *Ts'ung Pien*; and *tsung sha* (loc. cit. 4 lines above) should presumably be *ts'ung shu* 叢書. *Tsang Hua Lu* (p. 425) should be *Tung Hua Lu* 東華錄. *Liang Ch'ien Chi Wen* (p. 424) should presumably (?) be *Liang Ch'ien Chi Wen*, as on p. 342, note 82. The *Füeh Hsi-kuan chih* is wrongly attributed (pp. 315, 342, 425) to Yü K'un 預坤, instead of Liang Ting-nan 梁廷枬.

These corrections are trivial. After what has been said above it need hardly be reiterated that this is a work of first rate value.

T. K. FAIRBANK

Harvard University

## NOTES OF THE SOCIETY

The death of the following members has come to the attention of the Secretary:

Rev. FREDERICK C. EISELEN, Secretary of the Board of Education of the Methodist Episcopal Church, sometime Professor of Semitic Languages at, and President of Garrett Biblical Institute, who died May 5, 1937.

Professor EVA FRIESEL, formerly Research Assistant in Etymology at Yale University, and at the time of her death, May 28, 1937, Visiting Professor of Linguistics at Bryn Mawr College.

Professor A. V. WILLIAMS JACKSON, Professor Emeritus of Indo-Iranian Languages and Literatures at Columbia University, former President, and for many years Corresponding Secretary of the Society. A member since 1885, he died August 8, 1937.

Professor ROBERT K. REISCHAUER of Princeton University, killed at Shanghai on August 15, 1937.

The following have been elected to membership in the Society by the Executive Committee since the last meeting.

Zamir Ali  
James H. Breasted, Jr.  
Daniel S. Dye  
Mrs. Margaret Fairbanks  
Norman R. Gentry  
Robert A. Hall, Jr.

Maurice Jacobs  
Bertram S. Kraus  
George T. Kunitzoki  
Feng-Kuei Li  
Mrs. Edgar M. Morsman  
Richard A. Parker

W. B. Fettes	Benjamin Rowland
J. Prusack	Lakshman Sarup
Leo M. Reichel	Elizabeth A. Weiant

Not all of these have as yet qualified for membership.

The following have applications for membership pending:

Maynard E. Cassidy	Isaac Rabinowitz
D. G. Mandelbaum	Ralph W. E. Reid

Prof. Mary I. Hussey represented the Society at the Centenary of Mt. Holyoke College.

Dr. M. B. Emeneau will represent the Society at the Ninth All-India Oriental Conference meeting at Trivandrum in December, 1937.

On recommendation of the Committee on Arrangements (Professor I. G. Matthews, *Chairman*) the Executive Committee has voted that the next annual meeting be held on Tuesday to Thursday, April 19 to 21, 1938. The place of meeting is Philadelphia.

The attention of members is called to the fact that Volume X of the American Oriental Series, Dorothy Cocea, *Movable Property in the Near East Documents*, has appeared and will be available to them at the special rate of 55 cents until July 1, 1938. After that date the regular price of \$1.25, now charged to libraries and non-members, will obtain.

CARL H. KRAELING, *Secretary*.


Members of the American Oriental Society will be interested to learn of a four-hundred-page biography of a former president, *Talcott Williams: A Gentleman of the Fourth Estate*, by Elizabeth Dunbar (New York, 1936). The material is drawn from the original documents and records by one who was closely associated with his remarkable career. Born of American missionary parents in a picturesque village on Mt. Lebanon, Dr. Williams was educated in the United States and combined in a happy way the courtesy, sensitiveness, and retentive faculty of an Easterner with the efficiency, keenness, and industry of a Westerner. He soon rose to prominence in the world of letters. After working for years on the editorial staff of the *New York World*, the *Springfield Republican*, and the *Philadelphia Press*, he was appointed in 1913 first director of the Pulitzer School of Journalism, Columbia University, where he served until his death in 1928 at the age of seventy-eight. No less than eleven honorary degrees were bestowed on him. In the midst of his manifold and arduous activities he always found time to lift his voice and use his pen to the end that the Islamic world should become more widely and favorably known wherever the English language is used.

P. K. HERR.

# THE CITY AKTAB

ARNO POEBEL

UNIVERSITY OF CHICAGO

THE NAME of the Babylonian city ŠITI-TAK<sup>kl</sup>, known mainly from the Obelisk of Maništusu<sup>1</sup> and from texts of the third dynasty of Ur found at Drehem,<sup>2</sup> has been commonly read šit-tab<sup>kl</sup>. The reason for reading the first sign as šit was a very simple one, namely, the fact that this was a very common value of the sign . But in addition this reading seemed to be supported by the fact that it ended in *t*, while the following sign, tab, began with *t*. The name thus seemed to have a doubled or sharpened dental in its middle, a fact which reduced the number of its consonants to three.<sup>3</sup>

<sup>1</sup> *Délégation en Perse, Mémoires*, Vol. II, pp. 1 ff., Face D (Scheil), cols. 6 and 9. (For the correct lettering of the sides of the obelisk see p. 7 and *ibid.*, n. 2).

<sup>2</sup> For references see below, *passim*.

<sup>3</sup> Evidently for the same reason Scheil, when he published the Maništusu Obelisk, tentatively rendered the name with *ar-tas*<sup>kl</sup>. Note, however, that the value *rit* belongs not to the sign Thureau-Dangin, ROEC 419 (*sa-nu*, šiti, *ag*), with which the name of the city is written, but to the sign ROEC 303 (*mes*, *rit*, etc.).

For those who studied Hommel's *Grundriss der Geographie und Geschichte des Alten Orients* it will be interesting to recall that his speculative mind suggested to him the identification of the assumed Šittab with the city *Sittaca*, mentioned in Xenophon's *Anabasis* (II 4, 6), and later capital of the province of Sittacene (Ptolemaeus), on, or rather, near the Tigris, about 20 parasangs and 4 *stadia* (though probably by a considerable detour) southeast of Opis (according to statements of Xenophon). On the basis of his identification of Šir-ras<sup>kl</sup> with Sittake Hommel even ventured to propose (*op. cit.*, p. 346, n. 4) a reading šit-ta<sup>kl</sup> (in the index written *Schittach*) with final *š* (= *h*, German *ch*), instead of Šittab with final *b*, thus assuming an otherwise unknown phonetic value *ta<sup>h</sup>* for the sign *tab*. One wonders, of course, why he did not read the name directly šit-tak<sup>kl</sup> after the Greek Sittake. The reason was the following: In his *Geschichte Babyloniens und Assyriens* (1885), p. 283, Hommel had given a drawing of the impression of a seal in the Hague Collection, originally belonging—according to its inscription as reproduced in that drawing—to a prince of the city Šir-ras<sup>kl</sup>. Hommel, mistaking the sign šir for *ra* and reading the sign *ra* with its value *haš*, originally (*op. cit.*) read the city's name *la-haš*<sup>kl</sup> (in his own transliteration *Lugbaech*), believing it to be identical with the well-known *Lagāš* in southern Babylonia. But in his *Grundriss der Geographie des Alten Orients* (1904) he read the name *Sittar*, assuming that this was another form of the city name Šittab or Šittak. With the reading Sittab<sup>kl</sup> (= Šittak, Šittakh) with final spirantized *k* (= *h*, *kh*) he

In reality, however, the city's name is to be read *âg-tab<sup>k1</sup>*, as can be established from the inscription on a clay nail from the time of Sin-magir of Isin, acquired last year, upon my recommendation, by the Oriental Institute of the University of Chicago and entered in its catalogue under the number A 17650. The inscription reads:



## Col. I

1 <i>âg-tup-pi-tum-</i>	1 For Aqūuppitum
2 <i>âg-tab(a)bi-</i>	2 of Aktab,
3 <i>sin-a-ni-ir</i>	3 her <sup>a</sup> lady,
4 <i>nam-t[i]-</i>	4 for the life
5 <i>sin-ma-gir-</i>	5 of Sin-magir,
6 <i>lugal-kal-ga</i>	6 mighty king,
7 <i>lugal-lu-si-in<sup>k1</sup>-ma-</i>	7 king of Isin,
8 <i>lugal-ki-en-gi-</i>	8 king of Sumer and
9 <i>si-uri-k[a-š]é<sup>1</sup></i>	9 Akkad,

## Col. II

10 <i>nu-ū-up-tum-</i>	10 and for her <sup>a</sup> (own) life,
11 <i>munu-ša-ki-âg-</i>	11 Nuūptum,
12 <i>[.....]-ka<sup>a</sup>-ni-</i>	12 her [i] <sup>a</sup> beloved priestess
13 <i>[.....]ibila-na-ge</i>	13 .....
14 <i>šutum-</i>	14 .....
15 <i>ki-âg-gâ-ni</i>	15 built
16 <i>nam-ti-la-ni-šâ</i>	16 her <sup>b</sup> beloved
17 <i>mu-na-dâ</i>	17 šutummu

intended to give a form of the name intermediate between Sittak (with final tensile *â*) and Sittar (with final *r*—the palatal *r* according to him), while Sittab with final *b* is a fourth name form, belonging, as Hommel evidently reasoned, to a dialect in which final *k* or *g* became *b*.

I have referred at length to these details because they afford us a good opportunity to show how valueless are speculations based on arbitrary or insufficiently proved assumptions. For in other drawings of the Hague seal just referred to (Ménant, *Recherches sur la glyptique orientale*, Vol. I [1883], Fig. 21, and Ward, *The Seal Cylianders of Western Asia* [1910], No. 144, the latter republished in Weber, *Altorientalische Siegelbilder*, No. 215) the name of the city is reproduced not as *šir-*<sup>11</sup> but as *šir-*<sup>12</sup>, which to all appearances is our city *šir-tašš<sup>k1</sup>*.

<sup>1</sup> I. e., of Nuūptum.

<sup>2</sup> The restoration *-k[a-š]é* is made in accordance with the grammar of the classical period. Ll. 6 and 7 represent a double genitive construction ("for the life of the king of Sumer and Akkad") requiring the two genitive elements *-{a}k-a{k}*.

<sup>3</sup> Hardly sag.

<sup>4</sup> I. e., of Aqūuppitum(?). Or "his," i. e., Sin-magir's! Or is *ni* part of a proper name?

<sup>5</sup> I. e., of Aqūuppitum.

The name of the goddess Aqṭuppitum is Akkadian, as shown by the feminine ending *-tum* (= feminine element *-(a)t* + nominative ending *-u* + animation). The final *-itum* is doubtless the feminine gentilic ending (< *-ištum*), the whole name therefore meaning "she of Aqṭup" (in German: "die Aqṭuppische"). In all likelihood we have to supply an *ištār* before the name, the original name of the goddess thus being *ištār aqṭuppitum* "Ištār of Aqṭub," to be compared e. g. with *ištār* (= *\*xv*) *āš-ku-ri-tā* "Ištār of Aššur" (Schroeder, KAVI, No. 42, col. 2, l. 23; *bīt ištār āš-ku-ri-tā*, *ibid.*, l. 31). For omission of *ištār* in the Akkadian designation of the various Ištār's compare *\*ul-maš-ši-tum* "Ištār of É-ulmaš (a. g. in the name *warad-ul-maš-ši-tum*, Ranke, *Early Babylonian Personal Names*, p. 177, under 1-8), and especially the equations, Meissner, MAG I, 2, Text 1, col. 1, l. 21,

dinnana-kišī<sup>81</sup>

| kiš-ši-tum

where the Sumerian "Ištār of Kiš" is rendered in Akkadian with "the Kišite," and CT XXIV 50: K 4349 N (+ K 4349 U), col. 2, ll. 15 ff. + CT XXIV 20 ff.: K 4349, col. 5, ll. 1 ff. + Schroeder, KAVI, No. 145, obv. (!) (+ No. 73, obv.).

dinnana-nna (1)<sup>81</sup>

| b[e-lit nna<sup>82</sup>]

[dinnana]na-za-USLANUNU-UNU<sup>81</sup>

| s[u-.....i-tu]<sup>\*</sup>

[dinnana-k]i<sup>81</sup>

| kiš[-ši-tum]

[dinnana-.....]<sup>81</sup>

| sa-i[.....]

[dinnana-.....]<sup>81</sup>

| su[-.....]

.....

.....

dinnana-MALGA [...]

| [...]-ta-ma-[...]

dinnana-MALGA[...]...KUR[...]

| [m]a-LU(?)...-ka-[...]

dinnana-a-ga-de<sup>81</sup>

| be-šit ak-k[a-di-i]

dinnana-a-ab-bu<sup>81</sup>

| a-bi-i[ti]

dinnana-AB-ZU

| be-šit šit-a[...]

dinnana-aš(?)...-a

| ri-ta-i[-tu]

dinnana-URU-SIL<sub>3</sub>-MA

| šul-ma-ni-t[ti]

dinnana-a-ku-ur<sup>81</sup>

| u-ku-ši-t[ti]

dinnana-su-ba<sup>81</sup>

| su-bal-ti-tu

dinnana-su-ti-ti(?)

| su-ti-tu

dinnana-é-dam

| e-tem-me-tu

<sup>\*</sup> According to this unfortunately broken rendering of the Sumerian "Inna of ZA.USLANUNU-UNU<sup>81</sup>," the Akkadian name of the city began with *su*. This might well be the equivalent of *za* at the beginning of the Sumerian value of ZA-USLANUNU-UNU<sup>81</sup>. According to CT XII 28, col. 2, l. 26, this value was *za-ba-[-...]*, for which the older publication, 5 R 22, No. 1, gives *zā-bu-bu*.



where Ištar of Kiš is rendered as Kiššitum, Ištar of Akua as Akušitum, Ištar of Subal as Suballitu, etc.

Now, since according to our inscription the goddess Aqūppitum is worshiped at šir-tab<sup>11</sup>, the city of Aqūp, from which the goddess took her name, is evidently identical with that city, which should therefore obviously be read ak-tab<sup>11</sup> instead of šit-tab<sup>11,12</sup>

ug            | šir            | eš-qu

It will be noted that with the usual changes involved in a transition from Sumerian to Akkadian the consonants of the Akkadian name-form of the city correspond to those of the Sumerian name. Moreover, the first of the two vowels contained in the Akkadian name is the same as that in the Sumerian name. Only the second vowel has changed from *a* in Sumerian to *e* in Akkadian. But as I have shown in JAOS 57 (1937). 54 ff., this change is found, exactly as here, in other cases after a certain *d*. Compare Sumerian bad-du from bad-a, Sumerian muduru and Akkadian *gašurū* from Sumerian *gešdar*, Sumerian *mi-it-tu* and *mi-id-da*, Sumerian *domu-zī* beside Hebrew *tanmās*, etc. As I likewise pointed out in that study (p. 71), these changes or interrelations indicate a pronunciation of the supposed *a* as an open *o* in all these cases. That is to say, we have to read *baddo* instead of *baddu*, *modoru* and *gašorū* instead of *muduru* and *gašurū*, *domu-zī* instead of *domu-zī*, etc. Because of its interchange with *a* the *a* of Aqūp should therefore likewise be pronounced *o*; i. e., the actual pronunciation of the name was not Aqūp but Aqūp. Moreover, since in the cases enumerated above the dental preceding the *o* is frequently written with *t* (*to-mu*, *geš-to-ru-u*, *mi-it-tu*, and *bat-tu*), it was evidently pronounced as *t* in the name of our city, too. That, finally, as here assumed, the palatal immediately before the emphatic *t* was likewise emphatic, i. e., a *q*, is not quite certain but very likely.

According to Mr. A. Messayeh of New York, the dealer from whom the clay nail bearing our inscription was acquired, the object came from Abhari.<sup>13</sup> Upon my inquiry Mr. Messayeh further stated that it is situated "between Sunkara and Warka" (i. e.,

<sup>11</sup> The Sumerian value *ag* of the sign šir is well known from the equation in the syllabary CT XI 13, col. 4, l. 10:

<sup>12</sup> In one of his letters Mr. Messayeh gave the name as Ab Hari.

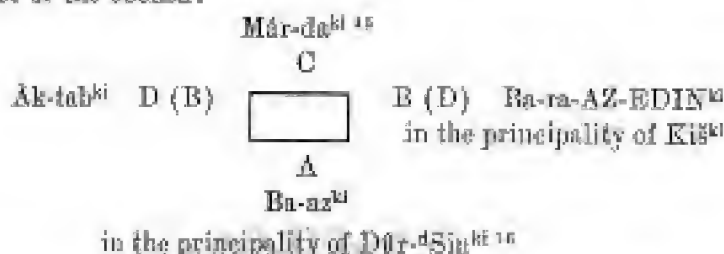
Larsa and Uruk), at a distance of about 10 to 13 miles from each.<sup>12</sup> Provided that both the provenience of the clay nail and the geographical position of Abhari are correctly stated, it would of course be tempting to assume that our Aktab was situated between the two south Babylonian cities. However, the evidence to be derived from occurrences of the former in the inscriptions can hardly be said to favor such an assumption. The city is mentioned several times in the third Ur dynasty tablets from Drehem, but it is not mentioned in the many contemporaneous tablets from Telloh, although the latter place is much nearer Uruk and especially Larsa than is Drehem, which is situated (according to Hilprecht) only half an hour's boatride southeast of Nippur. Note furthermore that in De Genouillac, *La trouvaille de Dréhem*, No. 81 (Year 42 of Šulgi of Ur), which records the receipt of fish and other food from different persons by a certain Drehem official, in one case instead of the usual phrase (. . .) x mu-du, "(so many fish) X has brought in," the more explicit phrase (. . .) šq-taš<sup>31</sup>-ta x mu-du, "(so many fish X has brought in from Aktab," is used. Although by no means impossible, it nevertheless does not seem very likely that someone would bring fish upstream to Drehem over a distance of about a hundred kilometers. On the other hand, the fact that the delivery of fish from Aktab is mentioned on the tablet in question together with deliveries by persons referred to merely by name—i. e., by persons who were well known at Drehem and evidently lived there or in its vicinity (Nippur)—makes it very likely that Aktab too was situated comparatively near Drehem.<sup>13</sup>

A similar position may, furthermore, be concluded from the

<sup>12</sup> In his letter Mr. Messayah referred to a book of Professor Dougherty which would give full information on the subject, but I have not been able to trace the statement to which he refers.

<sup>13</sup> The other instances where Aktab is mentioned in the Drehem tablets furnish no data bearing on the geographical position of the city. In De Genouillac, *Tablettes de Dréhem*, AO 5504 (5th year of Bur-Sin of Ur), the iššakku of Aktab (col. 2, l. 21) and eighty-four other persons (among them the iššakku's of Al-Sarraki, Kutha, Nippur, Abiak, Umma, Babylon, Asnunna, Kuzalla, and Lagaš) deliver one lamb each. Similarly in Nesbit, *Sumerian Records from Dréhem*, No. 14 (2d year of Bur-Sin), and in Fish, *Catalogue of Sumerian Tablets in the John Rylands Library*, No. 132 (4th year of Šulgi) the iššakku of Aktab delivers one lamb (in the latter case for the god Enlil), while in Fish, op. cit., No. 154 (44th year of Šulgi), he brings five kids.

Obelisk of Maništusu.<sup>14</sup> Since in cuneiform inscriptions places (countries, cities, etc.) as a rule are enumerated according to geographical principles, it may be assumed that the places mentioned on the obelisk are likewise enumerated according to such a principle. It will be remembered that each of the four sides of the obelisk enumerates fields belonging to one principality or to a district of a principality, with the possible exception of the front side, which perhaps enumerates fields belonging to two principalities. As far as the preserved cities are concerned, the arrangement of the inscriptions may be illustrated by the following sketch representing the top aspect of the obelisk:



Unfortunately, we know the precise position of only Kiš and Marad; but in addition we learn from the inscription on Face A that fields of the district of Bar adjoined the Tigris River, a fact which proves

<sup>14</sup> Although I am not quite sure that the following explanation of the king's name has not been given before, I wish to point out that the latter probably is to be conceived as *man'is-tisu* "A means by which one comes (or a certain person came) to life again (recovers or recovered from sickness) is (was) his incantation." The anticipation of the predicate is emphatic; cf. *šar-sis-tadad* "Forsooth, king is Adad."

<sup>15</sup> Mār-da<sup>ki</sup>, as the city here (and usually in Akkadian texts) is "ideographically" written, is the Sumerian name of Marad. The usual rendering of AMAR-DA<sup>ki</sup> as \*\*marad-da<sup>ki</sup> is of course wrong, since the sign AMAR has only the values amar (with stress on the second syllable) and mār, and never the value \*\*marad. For the development of Sumerian Marad to \*Mard (by dropping the final vowel) and finally (by inserting an auxiliary vowel) to Marad in Akkadian, cf. e.g. the development of Sumerian Nibru to Ni(p)pur in Akkadian.

<sup>16</sup> The traditional lettering (instituted by Scheil) of the sides of the obelisk from left to right (or, seen from above, in a counter-clockwise direction) is undoubtedly wrong. Since within the various bands on each side of the obelisk the writing moves from right to left, the side which adjoins the front side at its left should be the latter's continuation. From the above sketch it will be seen that only the lettering of the two narrower sides must be changed. In both cases the correct lettering is given in parentheses.

at least that Baz was situated east (possibly northeast or southeast) of Kiš. These few facts are not sufficient to decide beyond any doubt whether the places enumerated on the obelisk were situated in a more or less straight line or whether their enumeration describes a circle. But in either case Aktab must have been comparatively near Marad, which itself is situated about forty kilometers almost due west of Nippur. In case the enumeration followed a more or less straight line, Aktab would of course have to be sought somewhere south of Marad. If, however, the enumeration described a circle, the district of Baz might well have formed approximately the northeast corner, the district of Ba-ra-AZ-EDIN in the principality of Kiš the northwest corner, Marad the southwest corner, and Aktab then the southeast corner of the area in which the purchased fields were situated. This would actually bring the city of Aktab quite near the city of Nippur. If, finally, the purchased fields were not scattered over a very large area but were situated within comparatively easy reach of each other—this assumption would seem quite justified by the fact that the purchases are recorded on the same monument—the various estates may possibly have been located only in adjoining corners of the four districts somewhere in the region between Kiš and Nippur. In this case Aktab might even have been situated north or east of Nippur.

Since the *šutumma* of Aqsuppitu, according to our inscription, was built or rebuilt "for the life of Sin-magir, king of Isin," the city of Aktab must have belonged to the kingdom of Isin at that time. This again does not favor an assumption that the city was situated between the cities Uruk and Larsa, which were the capitals of independent kingdoms at that time. To be sure, this point cannot be stressed much, since we have no direct information concerning the events of Sin-magir's reign; nevertheless, a situation of Aktab nearer to Nippur would agree much better with the conception of the usual extent of the kingdom of Isin suggested by other known facts relating to the history of that period.<sup>17</sup>

<sup>17</sup> The city is also mentioned in two date formulas published by Stephens in RA XXXI 25 as Nos. 30 and 31 and reading:

mu ak-tab<sup>k1</sup> ba-hul

"Year (named): 'Aktab was warred against.'"

and

mu-us-sa ak-tab<sup>k1</sup> ha-hul

"Year following (that named): 'Aktab was warred against.'"

Unfortunately the king of the two date formulas is not known. Probably he belonged to the Isin or the Larsa dynasty.

The name borne by the authoress of the inscription occurs elsewhere in texts of the first dynasty—at least as far as they were utilized by Ranke in his *Early Babylonian Personal Names*—written exclusively nu-TU-up-tum and is registered therefore by Ranke (op. cit., p. 193a) as "nu-tu-ub-tum (with the note "cf. nutabu, Muss-Arnolt"). The writing with DU instead of TU in our inscription, however, shows that it must be read nu-tu-up-tum. It is derived from the root 𒌶𒌵 "to drop," "to drip"; compare Arabic *natafa* (present *yanṭuṭu* and *yanṭifu*) "to drop," "to drip"; *nutaṭum*, pl. *nutaṭun* "drop"; *nutaṭatun* and *nataṭatun* "drop" (= "earring"), "small pearl"; Hebrew *nāṭaf* (present *yāṭēf*) "to let (something) drip" (trans.); *nāṭāf* "drop," and *nāṭāfōt* "drops" (= "earrings"). The meaning of the name Nutuptum therefore is evidently "drop," either in the sense of "dewdrop" or, more likely, in the sense of "pearl," "eardrop," the latter meaning of course being derived from the former.

It is unfortunate that the lines immediately following the name are broken, since they would give us historically valuable information concerning the position of Nutuptum and possibly her relation to king Sin-magir. From what is preserved of the first sign in the line immediately after the name, it is quite clear that it is the sign *muṣ-iaṣ*, "priestess."<sup>18</sup>

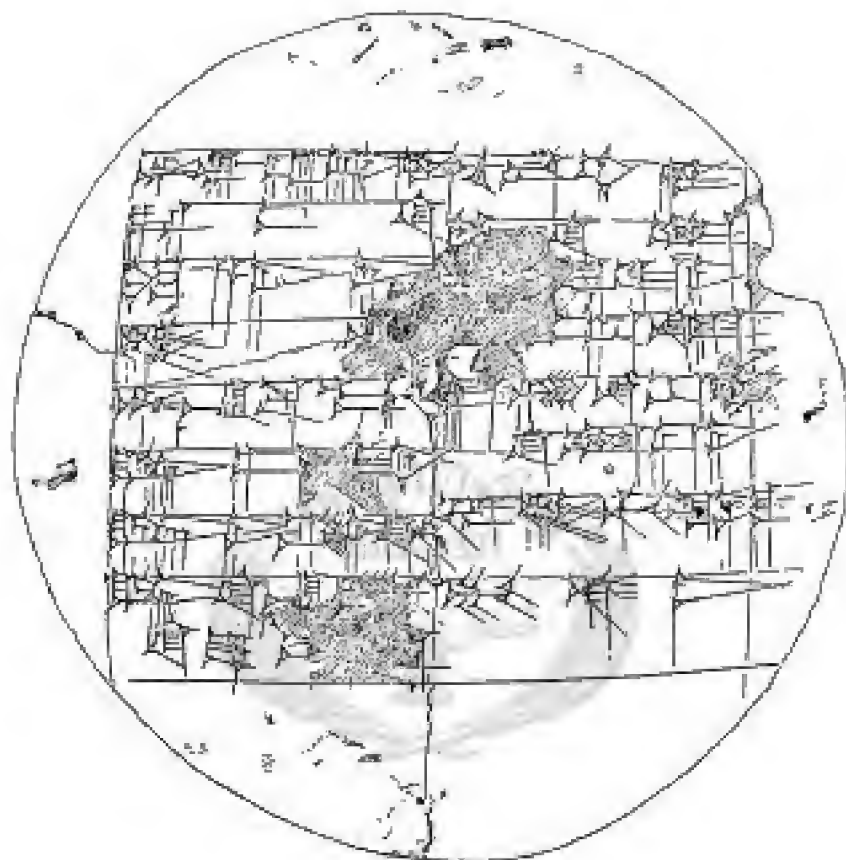
To all appearances, therefore, Nutuptum was a priestess, or rather—as is shown by her independent building activity—a high priestess, of Aquppitum. For a further restoration of the broken passage, however, hardly anything can be done with safety but to wait until a better preserved duplicate of the inscription is found.<sup>19</sup> Clay nails were usually inscribed in large numbers, and it is therefore likely that sooner or later such duplicates will come to light.

<sup>18</sup>It is therefore impossible to restore \*da[m]-ki-āg-[....]-KA(?) -NI-[iṣ-i]bīlā-na, "beloved wife of ....KA(?) -NI, his (i. e., Sin-magir's) brother(?) and heir," although there seem to be traces of wedge impressions in the deep break before *ibīlā* that might suggest the sign *iaṣ*.

<sup>19</sup>The following possibilities of a restoration may, however, be mentioned: "beloved priestess of (the god) ..., her ... and son"; "the beloved priestess who ...s for him (= Sin-magir) and for his brothers(?) and sons(?); the beloved priestess ..., the ... of his son." But for none of these suggestions can an exact parallel be found in the inscriptions. The words *-ibīlā-na-ge* conclude the subject chain beginning with *nu-tu-up-tum*; they represent the genitive *ibīlā-n(i)-ak*, "of his (her) son," plus subject-*a*.

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Head

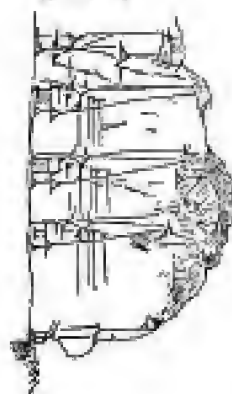


Skaft  
col. I

LI. 1-4



LI. 5-9



## WĒN TA-YA: THE FIRST RECORDER OF T'ANG HISTORY

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ONE OF THE turning points in Chinese history came in 617 A. D. with the Duke of T'ang's campaign from T'ai-yüan in Shansi to Ch'ang-an, the modern Sianfu, in Shensi. The empire of the short-lived Sui dynasty, which had reunited the whole of China for the first time in many years and had started a period of great cultural development, was rapidly disintegrating. Bandits and rebels had already gained control of large sections of the country. But it was reserved for the Duke of T'ang, Li Yüan, and his second son Li Shih-jian (T'ang T'ai Tsung) to take advantage of this situation and to establish a power which would gradually supersede the Sui authority and gain control over all sections of the country. Their rule was the beginning of the T'ang dynasty, the "Golden Age" of China.

The Duke of T'ang and his sons were in a position to make the best of the waning power of Sui Yang Ti (604-618). Briefly stated, their distinguished ancestry, the current prophecy concerning the future success of one of the Li name, their protected geographical location in Shansi, and Li Yüan's personal record of official service and recent military success, all aided in making him unusually prominent at this time.

Among the former Sui officials who looked to Li Yüan for leadership and flocked to join him even before he began his successful campaign through Shansi to the Western Capital, Ch'ang-an, was a scholar named WĒN TA-YA 溫大雅. He accompanied the future emperor on his march into Shensi and served him for several years in positions of great confidence and responsibility. He was with Li Yüan when Ch'ang-an was captured and he helped to arrange the ceremonies at the inauguration of the new dynasty in 618.

The Chinese literary works which are most commonly used as source materials for the history of this change in dynasty are three official dynastic histories: the *Sui shu*, *Sui History* or *Book of*

*Sui*, which was written by men who took part in the founding of the T'ang dynasty within fifty years after the events, and the *Chia T'ang shu* and *T'ang shu*, *Old and New T'ang Histories*, dated 945 and 1060 respectively, which are based on the official records and earlier histories of parts of the T'ang dynasty. Of these the *Chia T'ang shu* is the most important for the events of 617 and 618.

To these standard histories must be added the work of Wên Ta-ya. In his capacity as official recorder at Li Yüan's Headquarters he wrote a journal which is generally entitled the *Tu T'ang ch'uang yeh ch'i chü chü*<sup>1</sup> or *Court Journal of the Founding of the Great T'ang*. This is a chronological resumé of events concerning Li Yüan and his founding of the dynasty from 615 up to the proclaiming of the new emperor. It differs in many important details from the narratives of the standard dynastic histories. Of especial significance is the fact that it makes out Li Yüan to be the real leader and gives little to indicate that his son Li Shih-min was particularly important.

Most later accounts of the founding of the T'ang dynasty whether in Chinese or in western languages are based on the dynastic histories and stress the importance of Li Shih-min. This is done in spite of the fact that the *Court Journal* of Wên Ta-ya is recognized by Chinese scholars as containing reliable historical material. Hence there is a need for a critical study of this book in order that we may get a true understanding of the events which took place when the Duke of T'ang rebelled and took over the central authority at Ch'ang-an.

At the commencement of each of the three chapters of the *Court Journal* and immediately following the title itself, the author is referred to in this way: "Written by the T'ang minister, *ta hsing t'ai* of Shan-tung tao 陝東道大行臺, Great State Pillar, Duke of Lo-p'ing chün and of the State's Beginning, Wên Ta-ya." The titles "Great State Pillar" (*shang chü kuo*) and "Duke of the State's Beginning" (*k'ai kuo kwang*) were both honorary titles indicating his participation in the founding of the new dynasty.

The official biography of Wên Ta-ya is to be found in both of the T'ang histories. The version of the *New T'ang History* is obviously based on what had previously been written in the *Old*

<sup>1</sup> 大唐創業起居注.



*T'ang History* and hence the newer history is used here chiefly as a check in translating the older version.

The following is a translation of the biography of Wên Ta-ya from the *Chiu T'ang shu*, ch. 61, pp. 1a-1b<sup>22</sup> (parallel account in *T'ang shu*, ch. 91, pp. 1a-1b):

#### Translation

Wên Ta-ya, *tsü* Yen-hung 彦弘, was a man of Ch'î 祁 in T'ai-yüan. His father was [Wên] Chün-yü 君悠.<sup>23</sup> Under the Northern Ch'î he was a Scholar-Official of the Wên lin Academy 文林館. Under the Sui [dynasty] he was *ssü-ma* of Ssü chou 泗州 [in present Kiangsu]. At the end of *Ta Feh* [reign-period of Sui Yang Ti 605-617] he was Secretary in the Board of Justice [*ssü ü ts'ung shih* 司隸從事]. When he saw that the Sui government was daily becoming more confused he excused himself on account of illness and retired from office.

[Wên] Ta-ya by nature was most filial. In his youth he was fond of learning, and for able accomplishment his reputation was well known. [The *T'ang shu* version here adds: (Wên Ta-ya) with his younger brothers Yen-po 彦博<sup>24</sup> and Ta-ya 大有 were all well known. When Hsieh Tao-hêng 薛道衡 (an official under the Northern Ch'î, Northern Chou, and Sui dynasties; *Sui shu*, ch. 57) saw them he sighed in admiration and said: "All three men indeed have the abilities of high ministers." (This passage appears to be adapted from the *Chiu T'ang shu* biography of Wên Yen-po; ch. 61, p. 2a.)]

[Wên Ta-ya] held office under the Sui as Scholar-Official of the Eastern Palace [i. e. at the court of the Heir Apparent, who was at this time Yang Yung, a prince opposed and superseded by the Emperor Yang Ti] and as District Marshal of Ch'ang-an. On account of mourning for his father he retired from office. Later on because the empire was disturbed he did not [again] seek to hold office.

<sup>22</sup> *T'ung wên shu chü* photo-lithographic reprint of the Ch'ien Lung palace edition of 1730, Shanghai, 1924.

<sup>23</sup> *T'ang shu* ch. 61, p. 1a, gives 君攸, same pronunciation. *Chiu T'ang shu chiao k'ên chi* ch. 34, p. 15b, considers 悠 a mistake.

<sup>24</sup> He recorded *tsü* Ta-lin 大臨 may possibly have been his real given name.

When Kao Tsu [Li Yüan] was in charge of the garrison at T'ai-yüan he greatly honored [Wên Ta-ya]. When the "Righteous Troops" rose [in revolt], he brought in [Wên Ta-ya] to act as Staff Officer in charge of Records [*chí shih tsun chün* 記室希軍] at the Generalissimo's Headquarters. He had special control of official documents. At the time of the abdication and succession [i. e. of the last Sui emperor, Yang Yu (Sui Kung Ti 617-618), and Li Yüan (T'ang Kao Tsu) respectively] he and the Staff Officer in charge of Interdepartmental Affairs [*ssü lu (tsun chün)* 司錄 (希軍)] \* Tou Wei and the Chief Registrar [*chu po* 主簿] Ch'ên Shu-ta 陳叔達 together arranged the ceremonies.

During the first year of Wu Tê [618] he was transferred to be Vice-President of the Department of the Imperial Chancellery [*huang mén shih lang* 黃門侍郎]. His younger brother [Wên] Yen-po became Vice-President of the Department of the Grand Imperial Secretariat [*chung shu shih lang* 中書侍郎]. The pair occupied positions of close intimacy [with the Emperor Kao Tsu] and in [Imperial] deliberations they were honored. Kao Tsu in a relaxed mood spoke [to them] and said: "When I raised the Righteous Army at Chin-yang, it was only by means of you [two]."

After this he was transferred to the Board of Public Works. He was promoted to be *ta hsing tai* of Shan-tung tao and President of the Board of Public Works.

On account of the Yin Heir Apparent [Li Chien-ch'eng] and Prince Ch'ao-tz'ü [Li Yüan-chi] [these two were plotting against their brother Li Shih-min] T'ai Tsung [i. e. Li Shih-min] ordered [Wên] Ta-ya to take charge of the garrison at Lo-yang so as to anticipate [their] rebellion. [Wên] Ta-ya several times arranged secret plans and [thereby] received much official approval and rewards. When T'ai Tsung came to the throne he was successively transferred to be President of the Board of Rites and invested as Duke of the state of Li 黎國公.

When [Wên] Ta-ya was about to rebury his paternal grandfather, the diviners said: "Burial at this place brings the elder brother and brings happiness to the younger."

Ta-ya said: "If this brings about my younger brother's ever-

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\* The full title with the last two characters added is given in Tou Wei's biography, *Chien T'ang shu*, ch. 61, p. 2a.

lasting prosperity, I shall die with a smile on my lips." The burial was finished. After a little more than a year he died. His posthumous name was Hsiao 孝 [meaning "Filial"].

He wrote "*The Court Journal of the Founding [of the Great T'ang]*" in three chapters 卷.

In *P'ung Hui* 5th year [654, the 5th year of the T'ang emperor Kao Tsung] he was posthumously entitled: Right Vice-President of the Department of State Affairs [*shang shu yu p'u shé* 尙書右僕射].

This concludes the *Chün T'ang shu* biography.

There are a few further but mostly unimportant references to Wên Ta-ya in the early literature. Other biographies in *Chün T'ang shu* ch. 61 give statements containing no additional information and the section on bibliography merely states his authorship of the *Court Journal* as it appears in his biography.<sup>5</sup> Other items concerning Wên Ta-ya are contained in various Sung dynasty works: the *T'ang hui yao*, ch. 79, p. 18a; the *T'ai p'ing yü lan*, ch. 221; the *Tzū chih t'ung chien* of Ssü-ma Kuang, ch. 154, p. 2b; and the *Ts'ê fu yüan kuei*, ch. 722, p. 15b. These four all appear to be citations from the *Chün T'ang shu* biography and are important only inasmuch as they present variant readings of the original.

The *Ts'ê fu yüan kuei* is a collection of early documents published in 1013. Among them, in the section on "National History, (List of) Selected Works," is also included an item which throws a different light on one of the points in which we are most interested, the writing of the *Court Journal*. This is the statement: "Wên Ta-ya was President of the Board of Rites; he wrote *The Annals of the Establishment of the Present Ruler in his Royal Estate* (*Chün Shang wang yeh chi* 今上王業記) in six chapters."<sup>6</sup> This helps to confirm the authorship of Wên Ta-ya and from the unusual wording of the title one is led to believe that the *Court Journal* was written during the reign of T'ang Kao Tsu, i. e. Li Yüan, as emperor and before he abdicated the throne to his son Li Shih-mín.

<sup>5</sup> *Chün T'ang shu*, ch. 40 A, p. 31b.

<sup>6</sup> *Ts'ê fu yüan kuei* (1814 edition), ch. 556, p. 14a.

Another reference also indicates the early writing of the *Court Journal* and gives us what perhaps may have been the manuscript title while the events were being recorded and even before Li Yüan ascended the throne. In the *New Revised Gazetteer of the District of Ch'i* (this Ch'i was the native home of Wên Ta-ya), our scholar-official's biography is found in the section on "local worthies" and has been compared with the biographies in the dynastic histories.<sup>7</sup> It is identical in every way with the version of the *New T'ang History* except that the latter does not contain any reference to the *Court Journal* while at the end of the biography in the gazetteer is added this sentence: "He wrote the *Official Annals of the Great Chancellor, the Prince of T'ang* (*Chu Ta ch'êng hsiang T'ang wang kuan shu chi* 著大丞相唐王官屬記)." These personal titles were those assumed by Li Yüan a week after he had taken Ch'ang-an and at the time when he set up the boy Yang Yu as a puppet emperor, that is six months before he actually became emperor himself.

The various problems connected with the *Court Journal* cannot be fully discussed at this time. But it may be pertinent in connection with Wên Ta-ya's biography to add a little of what later scholars have thought about the date of his book. In a post-face to a Ming edition of the *Court Journal* the Ming scholar Hu Chên-hêng 胡震享 says that it was probably written while Li Chien-ch'êng (Li Shih-min's elder brother) was Heir Apparent, that is during the period 618-626, the reign of Li Yüan as emperor.<sup>8</sup> This also is the generally accepted opinion among Chinese scholars of the present day,—that Wên Ta-ya's book in its present form is an authentic document of the reign of T'ang Kao Tsu.

Wên Ta-ya could not have written much later than this time for he probably died within the next ten years, i. e. during the first part of the reign of T'ang T'ai Tsung. We have no dates either for his birth or his death and can only calculate them from what we know about his younger brother Wên Yen-po. The latter died in 637 A. D. at the age of sixty-four according to Chinese reckoning.<sup>9</sup> Hence he was born in 574 and his older brother some time prior to that date. Wên Ta-ya is not mentioned in the T'ang annals for

<sup>7</sup> *Hsin hsien Ch'i hsien chih* 新脩祁縣志 (1932), ch. 7, pp. 24a-25a.

<sup>8</sup> *Chin tai pi shu* 津逮秘書, Collection 19, Volume CX, Postface 3.

<sup>9</sup> *Chiu T'ang shu*, ch. 81, p. 3b, and ch. 3, p. 8a.

the reign of T'ai Tsung or his successor Kao Tsung and so any reckoning concerning his death must be based on the details of his biography and the dates of his brother. From the anecdote about the reburial of his grandfather and the prophecy of his brother's prosperity followed by Wên Ta-ya's death a year later it is probable that Wên Yen-po was still alive when the prophecy was made. The other brother, Ta-yu, had died in 618.<sup>10</sup> Hence Wên Ta-ya probably died some time between his last appointments, which were made after T'ai Tsung's accession in 626, and the death of Yen-po in 637.

Our scholar-official may then have died about the same time as his former master the first T'ang emperor whose death came in 635 nine years after his abdication of the throne. Their lives were bound together in many ways and to the extent that Li Yüan is an important figure in Chinese history so also must Wên Ta-ya's career be understood.

The latter's importance and that of his *Court Journal* is enhanced by the fact that he himself was a participant in the founding of the new dynasty. He was among the Shansi group of Sui officials who were with Li Yüan at the very start of the T'ang rebellion. As Recorder and keeper of official documents at the Duke of T'ang's headquarters Wên Ta-ya was on the inside of what was happening and Li Yüan himself later referred to him as an important member of those who shared in the responsibility for the success of the whole enterprise. At the time when he probably was finishing his account of the founding he was an official of high standing at the court and was personally trusted not only by the first emperor Kao Tsu but by his successor T'ang T'ai Tsung.

Such was the man who officially recorded the beginnings of T'ang history.

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<sup>10</sup> CH'U T'ang shu, ch. 61, p. 4b.

## SOME DETERMINING FACTORS IN THE NORTHWARD PROGRESS OF LEVI

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THAT THERE ever was a secular tribe of Levi remains largely a matter of conjecture. Certain it is that no existing tradition represents the Levites either as seeking to conquer a home for themselves or as occupying territory which they themselves had won. The outcome of that question need not concern the present discussion.

That the Levites, however, came to a pre-eminence throughout Israel, which made them unique among the tribes, is not a matter of controversy. Tradition says that their prestige was outstanding before the days of the monarchy. We know of them principally by their priestly prerogatives, which came to a complete formal triumph in the program of Deuteronomy.

Two things lend an abiding interest, especially to their earlier history. One is that the Levites are recognized as the formal sponsors and promoters of the leadership of Moses, the other that they are the advocates of the religion of Yahweh, and thus their growing ascendancy becomes only another way of gauging the rise of Yahweh worship to a place of dominance in Israel. Furthermore, that this would ever have come about without them seems highly improbable.

The process by which this result was achieved is nowhere clearly revealed. There are hints such as are found in Judges 17, which give insight in individual instances, though as in the reference just cited, the things that are assumed are often more fundamental than what is stated. For example, how the Levites had gained such a reputation in the period of the Judges remains a matter of speculation. Other unsolved problems such as their form of organization, how they became attached to Judah, how they were related to Moses, and how they became Yahweh worshippers need not detain us at this point.

Assuming their nomadic southern origin and their devotion to Yahweh, we are here primarily concerned with their progress northward into the settled land of Palestine. It is evident at once that this cannot be conceived in tribal terms. It appears to corre-

spend more nearly to modern religious propaganda, except that the Levite missionaries seem to become the sole custodians of the new cult. Back of this lies the traditional assumption that the Yahweh religion gradually displaced other religions in Palestine, as Israel gradually displaced other people. We know this picture to be false so far as Israel and the Canaanites were concerned. Israel's triumph was rather its amalgamation with other racial stock. What happened in the corresponding development of the Levites has not been clearly demonstrated. First of all our sources are less explicit on this point and it has seemed quite natural to assume that the Levites gradually took over all priestly functions.

This conclusion overlooks two basic facts: first, since the Canaanites were not wiped out, so neither were their local priesthoods. Our only cause for wonderment is what could have become of them; secondly, at the very point where the triumph of the Levites is complete, namely, in the formula of Deuteronomy that "all Levites are priests and all priests Levites," it is also equally clear that their predominant religion was not Yahwehism but Canaanite Baalism, and hence the demand for the Deuteronomic reform. This would be inexplicable on any theory of displacement of race or religion and requires a different conception of their history.

Much welcome light has been thrown on this problem by a thoroughgoing analysis of the traditional genealogies of the Levites ("Die Levitischen Überlieferungen des A. T. s," von K. Möhlenbrink, ZAW 11. 184-230) which were early articulated as a part of the genealogical scheme of the twelve tribes. As Möhlenbrink has forcefully pointed out, this material is very heterogeneous and cannot be wrought into a single unified system. It is possible, however, to establish stratification of sources and thus to distinguish earlier and later stages. In general, as in the other patriarchal families, order of descent indicates social rank at the time of compilation.

The things that chiefly interest us in this connection are the earliest scheme of Levitical genealogy and its probable dating. This is now found in Numbers 26:58 (LXX, and cf. Exodus 6:16-19): "These are the families of Levi; the families of the Libnites, the families of the Hebronites, the families of the Korahites, the families of the Mushites."

The verse occurs in a chapter that uniformly expresses its genealogical material in a form (*viz.* as gentiles) that suggests its early

origin. Verse 58 is cast in the same mould, but at the same time, in substance, it stands (minus the last clause) entirely independent of its context, and although all its elements occur in other Levitical genealogies, its family relationships are so different that it is impossible to harmonize the verse with its immediate context or with any other Levitical genealogy. This scheme is accordingly one that could scarcely have been formulated after the other existing systems had been arranged, while from its content its early primitive character is obvious.

In form Levi here consists of four clan families. They might all have been arranged as descendants of Levi, but such is not the scheme. On the other hand, in the corresponding list in Exodus 6, those four appear not as gentiles (clan families) but as persons, and three along with others, are declared to be grandsons of Levi, while the fourth is made a great grandson.

Of our primary group, the first two are identifiable locally with the Judean towns of Libnah (on Libni as "white," cf. Möhlenbrink, *op. cit.*, p. 196<sup>1</sup>) and Hebron.<sup>2</sup> The second pair cannot be identified with any definite place, though one can be located regionally. Here Korhi gives us Korah, a name that shifts widely in the genealogies and is entirely absent from some lists. Its early disappearance is accounted for in the legendary narrative of Numbers 16. The southern origin of the name is strongly supported, since Korah was one of the sons of Esau that "were born to him in the land of Canaan" (Genesis 36: 5, 14). Or, with I Chronicles 2: 42, he was a son of Hebron, i. e. Calebite in origin and therefore located in the neighborhood of Hebron. Many modern scholars (cf. ZAW 11. 196<sup>3</sup>) have been convinced that in the Mushites we have the Levitical group which championed the cause of Moses.

If these four classifications constitute the earliest known components of Levi, as seems highly probable, it becomes evident at once that the first three are indigenous appellatives of local priest-hoods, whether Judean or Edomite, that had taken on the epithet "Levite." This leaves only the Mushites as the original Israelite Levites and they must be found here or not at all. Moreover such a possibility cannot be denied.<sup>4</sup>

<sup>1</sup> Libni rather than Libnah is preserved in Exodus 6: 17, since the latter would have suggested not a son but a daughter.

<sup>2</sup> On Mushi as the gentile of Moses (Mosheh) cf. Zuph and Zaphai, I Chronicles 6. 28, 30.



While this greatly reduces the primary nucleus it also vastly simplifies the problem. The historic Levites are precisely defined by their relation to Moses. This result gains support and clarity from the only description we possess of the Levites' championship of Moses against a rival cult (Exodus 32:26-9). That scene distinctly gives the impression that the Levites not only took up the cause of Moses, but in doing so turned violently against members of their own kin (v. 29). It would appear then that only a minority of the earlier Levites joined forces with Moses. It would therefore have been particularly fitting that those who did should have been designated as the Mosaic Levites, i. e. the Mushites. Something which Moses communicated to them and which they promulgated, made both him and them renowned ever after. That thing, we believe, included the Yahweh cult and perhaps the oracle of the sacred lot.

As for the date of this earliest grouping, Möhlenbrink has suggested the broad probability that it was formulated between the time of Deborah and the kingdom of David. I believe that we can easily be considerably more explicit. At the time, the formal northern progress of the Levites had scarcely advanced beyond the east and west line of Hebron-Lachish. It was therefore very probably before the capture of Jerusalem by David and the establishment of Yahweh worship there. But the fact that Libnah precedes Hebron makes it highly probable that our list was formulated before David became king of Hebron, since after that event the priesthood of Hebron would certainly have been in the ascendancy and would have insisted upon its own primacy. Hence our list should be at least as early as the reign of Saul. By that time Yahweh worship had penetrated Ephraim and had spread to the Nebi'im. Shiloh was a center of Yahweh worship but the priesthood of Eli had probably not yet assumed Levitical status.

The order of the four divisions in our list is striking and presupposes a considerable lapse of time. Perhaps the most surprising thing is that Libnah should ever have been first. But its early outstanding importance is to be seen from the fact that to it was once ascribed a king of its own (Joshua 10:29), and also from its relation to the royal family of Judah as late as the days of Zedekiah (2 Kings 24:18). Its affinity and sympathy with the South and with Edom and its spirit of independence are evident from its

revolt against Judah at the time of the rebellion of Edom (2 Kings 8:22).

Sufficient time was indeed required for the transformation or at least the inoculation of the local priesthods of the Korahites and those of the larger towns of Hebron and Libnah with the Yahweh cult and further to allow a period of growth and development, to permit the stronger town priesthods to take the lead to such an extent, that the original Moasic group could have fallen back into a position of relative insignificance at the very end.

The bearing of this analysis is twofold. In the first place, looking forward, it makes clear for the first time the process by which the Levites became a part of what was to be Judah under Davidic rule. They were in fact an important element in its making, supplying as they did a somewhat extensive religious integration. And likewise they were strongly ensconced in Hebron before it became the capital; with David's expanding rule, their formal progress northward was inevitable.

Secondly, looking backward, this analysis has an important bearing upon the earlier status of the Levites. It has been repeatedly urged that the Levites were the only tribe that was in Egypt, partly if not largely because of the number of Egyptian proper names in the Levitical genealogies (Meek, *Hebrew Origins*, p. 33). Our results leave none to be considered in the oldest list. The name Moses, though Egyptian, raises no problem in the region south of Palestine, since it was known in the period concerned (to be sure in a compound form) both at the frontier of Egypt and as far north as Gaza (Olmstead, *History of Palestine and Syria*, p. 228). Moreover, Moses was not a Levite according to our oldest available sources, though the Levites later made him their honorary ancestor.

Of the remaining eight names cited by Meek, Noth (*Die Israelitischen Personennamen* 63 regards six as Egyptian. Möhlenbrink would allow most of these to come from the period of the divided monarchy, but with the possibility that one or more (e. g. Assir, found only in P and the Chronicler) may be of postexilic origin.

The later accretion<sup>3</sup> of Akkadian (Hur, so Noth), Arabic

<sup>3</sup> The argument from the presence of Egyptian proper names among the Levites, and their absence from the other tribes to show that the Levites alone were in Egypt seems to prove overmuch. It requires that the Levites carried not only in the Wady Tumilat (Goshen), which is the view of our

(Merari, so Noth), and Egyptian proper names appears to be more especially a Palestinian phenomenon that has its explanation in external rather than internal relations with Egypt and other lands.

The more precise problem is, why should people in Palestine at that time have adopted Egyptian or other foreign names. Priests might have been more apt to do so because of their wider contacts and knowledge, but it was not confined either to priests or Levites or limited to Egyptian, as the names Putiel, Aaron, Moses, Hur, and Merari appear to indicate.

The origin of the Levites remains in obscurity; and likewise their relation to Egypt, if any, is equally hidden from us. As the bearers of Yahweh worship, their northward progress is seen to have actually been the spread of a cult tradition, rather than the extension of a tribal group or a special class, and shows the earliest historic form of the Yahweh faith to have been primarily a priestly religion.

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oldest source (J), but they must have dwelt for perhaps generations in Egypt proper, so as to have intermarried and learned the Egyptian language. But if they had known things Egyptian so well they should have shown knowledge, now lacking, of much more than proper names after they came out.

The extent of Egyptian names among the Levites is best appraised in detail. (1) Putiel is a Semitic hybrid (Eg.-Semitic) and Noth properly excludes it from the Levites. Putiel was the father-in-law of Eleazer (Exodus 6: 24) and although his name occurs only here, there is no more reason for thinking him a Levite than in the similar case of Aminadab and Nahshon (v. 23), who are elsewhere listed as Judeans (1 Ch. 2: 10, Ruth 4: 10-20). He is not assigned to any tribe but is assumed to be an Israelite. (2) Hur is included by Meek as Egyptian, following Spiegelberg (OLZ 9, 160), but Noth refers this to Akkadian *huru* "kind." There were however several persons of this name. Noth emphasizes the Midianite chieftain (Num. 31: 8; Josh. 13: 21). There was also the contemporary of Moses (Ex. 17: 10, 12, etc.) and the skilled artificer (Ex. 35: 30), who in 1 Ch. 4: 1 is a Judahite, but in 1 Ch. 2: 19 a Calebite. I find no Levite of that name. (3) Merari is regarded by Noth as Arabic rather than Egyptian (op. cit., p. 325). (4) Aaron's name is included, though opinion is divided as to its Egyptian origin. In any case, he was not originally a Levite and stands in sharp antithesis to them as late as Ex. 32, as the champion of the northern bull cult. (Gressmann, *Moses und Seine Zeit*, p. 218.) (5, 6) The names of the brothers Kophni and Phineas (1 Sam. 1: 3) are recognized as Egyptian, but no tradition certifies them to be Levitical. If there is a presumption that this was once expressed in the text, at any rate if true it was probably of post-Davidic origin (see above). (7) The name Pashhur is first attested in the days of Jeremiah (Jer. 20: 1-3).

## THE LEMON IN ASIA AND EUROPE

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THE STATEMENT is frequently made that the lemon is a native of Arab lands, though when all the evidence is considered there is little or nothing which gives justification for this view. In the first place, all the Arabic names for citrus fruits are of foreign origin: these include *nāranj* (bitter or Seville orange, from Skt. *nāraṅga*), *darūqān* (sweet orange, from the name Portugal), and *atrūnj*, *turūnj*, etc. (citron, *Citrus medica*). This last appears in post-Biblical Jewish Aramaic as *ʾtrōgā*, *ʾtrāngā* (an affected pronunciation), *ʾtrūgā*, *ʾtrōg*, and *ʾtrōgā*; <sup>1</sup> the quotable Syriac forms are *ʾtrōg* and *ʾtrōgā*. These variations in consonantism and vocalism, as well as the non-Semitic form of the word, stamp it as a borrowing from an outside source, most probably a Middle Persian *\*atrūng* (cf. the arabicized NP *atrūnj*). In view of the priority of the Aramaic forms it is quite possible that the Arabic are, as Fraenkel <sup>2</sup> believes, derived from them rather than directly from the Persian. The Persian derivation of the Aramaic held by Dalman (op. cit.) is moreover corroborated by the Latin name for the fruit, *citrus medica*, as well as by the Syriac *ḥaṣṣā māḡḡā*.

Secondly, no Arabic author, as far as I can discover, says that any species of the genus *Citrus* is native to the Arabian peninsula. Al-Ḥamdāni († 945-6), himself of South Arabian origin, in his *IKHL*,<sup>3</sup> which treats of the history and antiquities of South Arabia, suggests a systematic importation of all kinds of fruit from India to South Arabia even in pre-Islamic times. His statements are given strength by Bent's account<sup>4</sup> of the district around Dhofar

<sup>1</sup> Gustaf Dalman, *Aramäisch-neuhebräisches Handwörterbuch*, 2nd ed. (Frankfurt a. M., 1922). M. Jastrow (*A Dictionary of the Targumim*, London and New York, 1903) would derive *ʾtrōg* from *ʾtrug-šurug*, to be bright, regarding *šurug* as a *šaf.* of *ḥurug*, to break through. This tortured etymology completely breaks down when *šurōgā*, *šuraggā*, of which this supposed verb is the denominative, is seen in its true light as a Persian loan-word; cf. NP *šūrōgh*.

<sup>2</sup> *Die aramäischen Fremdwörter im Arabischen* (Leiden, 1896), p. 139.

<sup>3</sup> Ed. A. Karmali (Baghdad, 1931), pp. 32, 182.

<sup>4</sup> "Exploration of the Frankincense Country, South Arabia," *Geographical Journal*, VI (1895), p. 114.

(Zafar), which was the center of trade between Hadramaut and India, especially Baroch, in the period around the beginning of the Christian era: "Along the whole line from Ra's Riut to Mirbat are many groves of coconut palms . . . in the gardens we find many of the products of India flourishing, namely, the plantain [Ar. *namas*, from Skt. *maca*], the papaya, mulberries [Ar. *tât*, from Pers.; Skt. *tāda*, *tāla*, is a l. w. from Pers.] melons, chilis, brinjoles and fruits and vegetables of various descriptions." The first Muslim author who mentions the lemon (ليمونة), pronounced in classical Arabic either *līmūnah* or *laymūnah*, the *-ah* being the feminine singular ending)<sup>5</sup> leaves little doubt that it was not a fruit familiar to the Arabs of his day and ascribes it explicitly to al-Manṣūrah, a port and southernmost Muslim outpost in Sindh: "They have the sugar cane, and in their lands is a fruit the size of an apple which is called the ليمونة; it is sour and very acid (حامض شديد الحموضة)."<sup>6</sup> Ibn-Hawqal's (mid 10th century) notice may be passed over, since he merely repeats what al-Iṣṭakhri says.

By far the most comprehensive account of the lemon in Arabic sources is the treatise of the Jewish physician ibn-Jamī, a member of Saladin's (1171-93) court and one of the most respected practitioners of his day. His dissertation on the lemon was one of his best-known works,<sup>7</sup> and though now lost in the original has been fortunately preserved in an *in extenso* quotation in ibn-al-Bayṭār's († 1248) *Al-Kitāb al-Jāmi' fi al-Adwiyah al-Mufrada*,<sup>8</sup> a well-known pharmacopoeic work: "Le limon se compose de trois parties qui diffèrent de qualités d'emploi: l'écorce, la pulpe (ou la partie acide) et les graines. L'écorce, quand on la mâche, a beaucoup d'amertume, une légère âcreté et une astringence latente. Elle a, de plus, une aromaticité manifeste, ce qui prouve qu'elle est d'une chaleur<sup>9</sup> à peu près tempérée et d'une sécheresse évidente. Elle est chaude au commencement et sèche à la fin du second degré. En vertu de son amertume, de son astringence et de son aromaticité,

<sup>5</sup> Al-Iṣṭakhri (first half 10th century), *Masalik al-Mamalik*, ed. M. J. de Goeje (Leiden, 1870), p. 173.

<sup>6</sup> Ibn-abi-Uayyih, *'Uyūn al-Anbā' fi Tabaqat al-Arab* (Cairo, 1882), p. 115.

<sup>7</sup> Tr. Leclerc in *Notices et extraits des manuscrits de la Bibliothèque Nationale*, XXVI (1893), pt. 1, pp. 255-62. Sontheimer's translation is inaccurate.

<sup>8</sup> This and following similar terms are used in the sense of the old Greek humoral pathology.

elle fortifie l'estomac. Elle a la propriété d'exciter l'appétit, d'aider à la digestion, de parfumer l'haleine, d'exciter et d'assainir les viscères, de fortifier le cœur et de rectifier les humeurs malsaines. C'est aussi un antidote contre l'action des poisons et des haves infectées de virus. Voilà pourquoi elle est considérée comme médicament. Comme aliment, elle est indigeste, passe lentement et nourrit peu, ce qui tient à ce qu'étant dure et résistante, elle est difficile à mâcher. Sa saveur et son odeur persistent longtemps dans les viscères. Le limon est jubeux. On le mange après l'avoir dépouillé de son écorce extérieure et jaune, jusqu'à ce qu'il soit mis au nu et recouvert seulement de cette pellicule mince et blanche qui ressemble à celle de l'œuf. On peut l'exprimer aussi en conservant son écorce." He continues with mention of its use as an agent in bleaching the skin and in cleaning copper, while medically it was employed as a specific against excessive bile, nausea, serpent and scorpion bites, and as a substitute for vinegar; many of these uses are still common. Reference is also made to *al-limūn al-wurakkab*, or lemon grafted to the citron tree.

Jacques de Vitry († 1240),<sup>9</sup> bishop of Acre, describes a fruit common in Palestine which is certainly *Citrus medica* limon: "Sunt præterea aliæ arbores fructus acidos, pontici videlicet saporis, ex se procreantes, quos appellant limones. Quorum succo in aestate cum carnibus & piscibus libentissimè utuntur, eo quòd sit frigidus & exsiccat palatam, & provocans appetitum." The use of the juice of the lemon as a condiment is characteristic of this fruit in contradistinction to the citron. The well-known geographer Yāqūt († 1229)<sup>10</sup> makes special notice of the abundance of both lemons and citrons growing in the Wādī al-Jarmaq, in the district of Sīdūn.

In his *Kitāb Athār al-Bilād wa-Akkhār al-Thād*,<sup>11</sup> Zakariyā' ibn-Muhammad al-Qazwīnī († 1283) says that "Al-Manṣūrah is a well-known city in Sindh . . . In it there are two fruits not found in any other city: one of them is the ليمون [in classical Arabic pronounced *līmūn* or *laymūn*], which is the size of an apple."

Ibn-al-Wardī († 1457)<sup>12</sup> notes that "The ليمون [*līmūn*, *laymūn*]

<sup>9</sup> "Historia Hierosolimitana," in J. Bongars, *Gesta Dei per Francos* (Hannu, 1611), p. 1090.

<sup>10</sup> *Kitāb Mu'jam al-Bulad*, ed. F. Wüstenfeld, II (Leipzig, 1867), p. 64.

<sup>11</sup> Ed. Wüstenfeld (Göttingen, 1849), p. 83.

<sup>12</sup> *Kharidat al-'Ajd'is wa-Faridat al-Gharā'id* (Cairo, 1302), p. 100.

is an Indian plant which does not grow well and flourish except in hot countries." The rest of his notice is a repetition of its medicinal properties as given by ibn-Jamāl.

The citron, which ibn-Jamāl and others clearly distinguish from the lemon was, as we have seen, known to the Arabs under the name *utrunj*, *turunj*, etc. Lane defines this as "A certain fruit, well known, plentiful in the land of the Arabs, but not growing wild [of the species *citrus medica*, or citron, of which there are two varieties in Egypt; one the form of the lemon, but larger, called *نرج بلدي* (native citron); the other, ribbed, and called *انرج مصع*; according to Golius, citrons of a large size, which have a sweeter peel than others, and are of a size nearly equal to that of a melon:] . . ." The route by which a certain variety of citron and the orange reached the West is given by al-Mas'ūdī († 956) in his *Murāj al-Dhahab*:<sup>12</sup> "Indike manner trees of the orange and of the round citron were imported from the land of India after the year 300 [of the Hijrah] and cultivated in 'Umān.<sup>13</sup> Then they were brought to al-Basrah, al-'Irāq and Syria until they became plentiful in people's houses in Tarsus and other localities on the Syrian frontier and in Antioch and the coastlands of Syria, Palestine and Egypt; before this they were entirely unknown. But they lost the pleasant winy aroma and good color they had in India, owing to the lack of the air, soil, water and peculiar properties of that land." It is very likely that the lemon, too, reached Palestine and Syria by this route.<sup>14</sup>

It appears from the adduced passage from al-Iṣṭakhri that *ليمونة* (of which *ليمون* is the collective) is the oldest quotable form of the word for lemon in Arabic. The spelling *ليمر* is also old, though apparently somewhat more common in Persian than in Arabic. According to ibn-Hawqal (wrote ca. A. D. 977)<sup>15</sup> the speech of al-Manṣūrah was both Arabic and Sindhi. But Yāqūt, who in his article on al-Manṣūrah used al-Iṣṭakhri, calls a fruit which from the other versions must be the *laymān*, [*al-lhamarah*]

<sup>12</sup> Ed. and tr. E. de Maynard and P. de Courteille, II (Paris, 1863), pp. 438-9. This passage has, owing to the ambiguity of the *citron* of the French translation, often been mistaken as referring to the lemon.

<sup>13</sup> See al-Qazwīnī, op. cit., p. 196.

<sup>14</sup> Indicated by an anecdote concerning the lemon in al-Qazwīnī's *'aḥ'āṭ al-Makhṭūṭ*, ed. Wüstenfeld (Göttingen, 1846), p. 266.

<sup>15</sup> *Maṣālik al-Mamālik*, ed. de Goeje (Leiden, 1872), p. 232.



*al-baklawīyah*, the Pahlavi (Persian) fruit. Since al-Iṣṭakhri nowhere mentions this fruit as growing in Persia, we must conclude that it was brought to Sindh by Persian merchants, and its localization in a maritime trading center such as al-Manṣūrah leads us to suspect that it was brought thither from further east by sea. An identical case is that of the Arabic name *bartuḡān*, referred to above, which was given to that fruit not because it came from Portugal itself, but because it was introduced by Portuguese traders from the Far East.

The Arabic word for lime, *limah*, is rare and late in literature and is cited, among the dictionaries, only by Dozy, whose work is composed of such words. The earliest reference to the lime which I have been able to find in Arabic writers is in ibn-Baṭṭūṭah's († 1377) *Takfāt al-Nuṣṣār fī Ghara'ib al-Am̄ ar wa-'Aḡā'ib al-Aṣfār*, 4th impression, III (Paris, 1914), p. 126. In the two pages he devotes to the fruits of India here he speaks of the mango and says that the Indians preserved it in salt and brine "as the lime (*limah*) and lemon (*limān*) are pickled in our country." It is noteworthy that he does not mention the lemon along with the fruits of India. The lime, it is generally agreed,<sup>17</sup> is a typically Indian fruit, and it is undoubtedly from India, or more accurately Ceylon, with which the Arabs had commercial relations that antedate even Islam, that it passed to the Arabic language. Bonavia<sup>18</sup> mentions the name *lima* as applied to a species of *Citrus* in Ceylon; in Arabic (according to Dozy) *lim* (sg. *limah*) means the lime, while *lim ḥalaw* or *limjīnā*, and *lim qārīṣ* mean the sweet orange and lemon (*citron*) respectively. This shows it must have been loosely used in the general sense of *citrus*, somewhat like the French *citron*. From the Arabs the word *limah* passed on to Europe, where it became Italian, Portuguese, and Spanish *lima*. English *lime*, from French *lima*, does not appear until 1638 (*Oxford English Dictionary*). The French word is from Late Provençal *limo*.<sup>19</sup> On the other hand, Italian *limone*, Portuguese *limão* and Spanish *limón* are considerably earlier. Middle English *lymon*, from Old French from Provençal *limoun*, occurs as early as ca. 1400. The modern French

<sup>17</sup> George Watt, *The Commercial Products of India* (London, 1908), p. 326; E. Bonavia, *The Cultivated Oranges and Lemons, etc., of India and Ceylon* (London, 1888), p. 80.

<sup>18</sup> Op. cit., pp. 81-2.

<sup>19</sup> E. Gamillscheg, *Etymologisches Wörterbuch der französischen Sprache* (Heidelberg, 1929).



blanket use of the term *citron* for the citron, lemon and lime appears to be due to the circumstance that the dialect of the Ile de France, which forms the basis of modern literary and standard spoken French, was affiliated with the dialects of northern France, where lemons, and later limes, were so little known that *citron* was felt to suffice for all three. This condition originally existed also in English, which at first adopted the French usage, but later relegated *citron* to its proper place. Germany, however, which borrowed its *Citronen*, *Zitronen*<sup>20</sup> from Italian before the middle of the sixteenth century and was never so active in importing oriental foodstuffs as England and Italy, still retains the old ambiguous usage.

About the history of the lemon in India itself: The St. Petersburg Lexicon lists over fifty words which are defined as *Citrona*, *Citronenbaum*, or varieties thereof. They are nearly all descriptive terms, and many have other meanings as well; practically all of them are bookish words occurring only in dictionaries. Of all these, the only one which could possibly have any connection with the Arabic *limūn*, *limū*, etc. is *nimbā* and its compounds, which is first recorded in the *Rājānighaṇṭa* (dating from 1235-50). This is 300 years later than the earliest mention of the *limonak* in Arabic literature and about 100 years after the first extensive notice of the *li-mung* in China.<sup>21</sup>

Uhlenbeck<sup>22</sup> regards *nimbā* as a "ganz junge sanscritisierung von hind. *limū*, *nimbā*". . . Derselben quelle entstammen up. *limā*, etc., welche man früher mit unrecht auf *nimbā*- zurückgeführt hat. Der eigentümliche lautgestaltung von hind. *nimbā* lässt sich durch volksetymologische einwirkung von *nimbā*s erklären." He regards *nimbā* as a word of Malayo-Polynesian origin and compares Dayak *limau*, Javanese *limo*, and Malay *limau*. Turner<sup>23</sup> cites Nepali *nību* or *nību*, Kumaoni *nīmuwā*, Assamese *nemu*, Bengali *nēbu*, Oriyā *nembu*, Hindustani *nību*, Panjabi *nimbā*, Gujarati *libu*, and Marathi *nībū*. He believes that in Sindhi "it appears to be con-

<sup>20</sup> F. Kluge, *Etymologisches Wörterbuch der deutschen Sprache*, 11th ed. (Berlin and Leipzig, 1934).

<sup>21</sup> Cf. B. Laufer, "The Lemon in China and Elsewhere," *JACS* 54 (1934): 145.

<sup>22</sup> *Kurzgefasstes etymologisches Wörterbuch der altindischen Sprache*, 2 vols. (Amsterdam, 1898).

<sup>23</sup> R. L. Turner, *A Comparative and Etymological Dictionary of the Nepali Language* (London, 1931), s. v. *nību* (p. 348).

fused with Sk. *nimbāṭṭ*, *Azadirachta indica*: *nīmu*, *limu*, but *limo*, lime.<sup>21</sup> Vullers<sup>22</sup> thinks that perhaps Pers. لیمو (*limā*, *limō*) may be cognate with Skt. *nimbāṭṭa* (his *nimbāṭṭa* should be corrected to *nimbāṭṭa*). To these may be added: Hindi *nimbā*, *nimbā*, (which Platts regards as the standard form; he cites also *nīmā*, *nībā* [whence Pushtu *nībā*, lemon, lime], *limā*, *lemā* and *lembā*); Bengali *lebā*; Oriya *nimbā*; Balochi *nīmā*, *limā*; Kashmiri *nyomā* (dat. eg. *nēmā*); Santali *lemā*, with which Bodding<sup>23</sup> compares Bengali *nimbā*, Hindi *līmā*, *lemā* and *nimbā*, and which he appears to consider a loan-word from Hindi *nimbā*. These forms all seem to be derivable from an original \**nimbāṭ* meaning lime (thus do Wilson and Monier-Williams define Skt. *nimbāṭṭa*), but later applied to the lemon when that became known; the present state of confusion between the lemon and lime in India has been pointed out by Miss Johnson in a recent article in this journal.<sup>24</sup> The Ceylonese word *lima* (whence probably Arabic *limā*) is closely related to, if not actually borrowed from, Sindhi *limā* and Hindi *limā*, *lemā*.

The Perso-Arabic *limā* presents a very difficult problem to the etymologist. Some Arabic lexicographers<sup>25</sup> regard this as arising from the dropping of the -a of *limāna*, but it might also be argued that it could be a borrowing from Hindi or Sindhi *limā*, *lemā* (which seems to be the case with Balochi *nīmā*, *limā*). It is pretty clear, however, that the Indian words refer to the lime and not the lemon; moreover, both Hindi and Sindhi cannot be beyond the suspicion of having been influenced by the Persian. Since the lime is by common consent native to India, being found in a wild state in the forests of the southern slopes of the Himalayas, especially in the valleys of Kumaon and Sikkim,<sup>26</sup> I should be inclined to regard Ar. *limā*, lime, as of Indian origin (non-Indo-European) and *limā* as possibly so, but more probably a shortening of the Perso-Arabic *limāna*.<sup>27</sup> It should be noted, moreover, that Persian *limā* is a very widespread word; not only has it been borrowed into

<sup>21</sup> *Lexicon Persico-Latinum*, II (Bonn, 1804).

<sup>22</sup> *A Santal Dictionary* (Oslo, 1920).

<sup>23</sup> "The Lemon in India," *JAOS* 56 (1936), 47.

<sup>24</sup> *Tāj al-'Aras* (from al-Jawhari); *Muḥīṭ al-Muḥīṭ*.

<sup>25</sup> Cf. Laufer, op. cit., p. 156, who here mistakes the lime for the lemon.

<sup>26</sup> For dropping of final -a after long vowels in Persian see Paul Horn, "Neupersische Schriftsprache," in *Grundriss der iranischen Philologie*, I (Strassburg, 1898-1901), p. 58.

Arabic, Turkish, and Kurdish as *limā*, *limō*, but even made its way to China, where it appears as *li-mu* in the Yüan (Mongol) period.<sup>20</sup>

Mr. Edwin H. Tuttle<sup>21</sup> has proposed that the Sanskrit *limba* may have come from a Dravidian *\*limbās*, which might be an older form of *\*limbas*, the form he assumes for Kanarese, Tamil, Telugu, and Tulu words for lime and lemon. But a better explanation is to derive the Dravidian words from Sanskrit, which has loaned for example *jambhara*, lime (see below, p. 600) to this group. Mr. Tuttle's etymology is hypothetical, and whereas his *\*limbās*, *\*limbas* are starred forms, the Sanskrit *limbā* was in common use. The botanical evidence, finally, is against South India as the locale of the introduction of the lemon into India.

The *Silsilat al-Tawārikh*<sup>22</sup> of abu-Zayd al-Ḥasan al-Sīrāfi, the first part of which was composed by Sulaymān al-Tājir, enumerates in this first part, which was written in A. H. 237 (A. D. 851), various fruits of India, but says nothing of the lemon. Likewise, in the section dealing with China the citron (*utruf*) is mentioned, but not the lemon, though the argument *a silentio* cannot be too heavily leaned upon in either case. The Arabic sources, as has been seen, localize the lemon in Sindh. Watt (op. cit., p. 325) believes that the wild form of the lemon is not known in India. He further says that as a rule lemon culture in India is confined to gardens (as it was in Arab lands) and is hardly one of the regularly grown fruits (p. 326). Miss Johnson (op. cit., pp. 47, 50) agrees that the lemon is rare in India and of foreign origin. Dr. Swingle<sup>23</sup> also believes that the lemon is not native to India, as does Bonavia (op. cit., p. 240), at least as far as the name, and thus presumably also the fruit, is concerned. From the above material, then, it appears that the lemon came to India from some foreign source, and that it was brought to Sindh by Persian, possibly non-Muslim, traders in the ninth or tenth century.

The Malay origin of the word for lemon is held by Bonavia (op. cit., p. 240), Uhlenbeck (loc. cit.), I. H. Barkill,<sup>24</sup> H. Kern,<sup>25</sup>

<sup>20</sup> Laufer, op. cit., p. 150.

<sup>21</sup> Idem, p. 147, n. 6.

<sup>22</sup> Ed. Reinaud (Paris, 1811).

<sup>23</sup> In Helen M. Johnson, op. cit., p. 47, n. 3.

<sup>24</sup> A Dictionary of the Economic Products of the Malay Peninsula, I (London, 1935), p. 569.

<sup>25</sup> Art. "Limoen," *Verspreide Geschriften*, XII (Copenhagen, 1924), pp. 152-3.

and T. Tanaka.<sup>26</sup> Of these, Kern is the only one who approaches the problem from a philological standpoint, and he has been severely criticized by Laufer (op. cit., p. 156). Kern notes that Hindustani has *limā* and *nimbā*, while a quasi-Sanskrit *nimbāka* is of late occurrence. *Nimbāka*, he claims, is a Sanskritization of Hindustani (sic) *nimbā*, which in turn is a corrupt pronunciation (*sene bedorrens nītaprak*) of *limā*. According to Kern this word was borrowed from the speech of the Indian Archipelago and Formosa: Ngadju-Dayak has *limau*, which means citron, lemon or shaddock (*pompelmoes*); it is not a Portuguese loan-word, as some dictionaries say. He states that the form *limā* appears in the Old Javanese Rāmāyana about 500 years before contact with the Portuguese. The Old Javanese *limā*, he points out, arose from an older *limau*; therefore *limau* must have been known in the Indian archipelago in the early Middle Ages. He cites Formosan *lima*, Sundanese *limo*, Balinese *lima*, Nias *dima* and Makassar *limo*.

This, however, still makes the first reference to the lemon (if it really is the lemon) in Old Javanese literature only about contemporary with its first extensive mention in Chinese works and at least a hundred years later than its first citation in Arabic. From the list of citrus fruits of Malaya which Burkill<sup>27</sup> has compiled it is evident that the word *limau* is used in Malay in the general sense of citrus, the varieties being distinguished by the use of qualifiers. Following Tanaka, Burkill distinguishes two varieties of lemon: *C. limon*, Burm. he believes to be of Indian origin; he states that it has been experimented with in Malaya but that it has no place in the markets against the true lime. This is the common, or European, lemon. *C. limonia*, Osbeck, the Canton lemon, he says has a name which is "obviously Malayan in origin," and suggests that the species came from India via Malaysia about 700-800 years ago. He adds that races persist in Malaysia. Marsden,<sup>28</sup> on the other hand, lists *limau* and its variant *limān* as Persian loan-words. With regard to the second of these forms he is undoubtedly right, since this is typically Islamic; its sole occurrence in India is in the

<sup>26</sup> "On Canton Lemon," *Bulletin de la Faculté de Lettres, Université Impériale de Tokyo* (1924-5), p. 125.

<sup>27</sup> "An Enumeration of Paramignya, Atalantia and Citrus, found in Malaya," *The Gardens Bulletin, Straits Settlements*, V, nos. 7-8 (Singapore, 1931), p. 215 ff.

<sup>28</sup> *A Dictionary of the Malayan Language* (London, 1812).

Dakhani Hindustani *līmān*,<sup>32</sup> where it can only be regarded as a loan from Persian. Moreover, Burkill himself is disinclined to consider Malaysia as the home of the lemon, whether the Canton or the European variety (a distinction which not all botanists agree with him and Tanaka in making), and his supposition that it came from India we have seen to be unlikely. The widespread occurrence of the word *līmān* in the Malayan tongues cannot be taken as positive proof that it originally meant lemon, for it is applied to many other kinds of citrus fruits, nor that it is even native to these languages, since in the last 500 years the word has spread even more widely from Arabic to European and American tongues. In view of these considerations it is permissible to ask whether the Malay use of *līmān* for any variety of *Citrus* is not analogous with the French use of *citron* for citron, lemon and lime, with the modern Indian use of *līmā* for both lemon and lime, and the Indian "*pahari nimbu*, *karna nimbu*, *meta-limbu* and *thara-limbu*," all meaning lemon (or varieties thereof).<sup>33</sup>

Watt mentions another name for lemon which he gives as *kalembak* in Arabic and *kalimbak* in Persian. This must be the *kalambo* referred to by Bonavia,<sup>34</sup> to whom the meaning is uncertain; he thinks it may be a pumpkin-like citron. Most probably the fruit here meant is the carambola (*Averrhoa carambola*), whose name came into English via Portuguese from Marathi *karambal*, Skt. *karmara*; the Malay *kérambíl* is in all likelihood of Indian origin. Dozy gives the Arabic *qalambaq* as being from Malay; the borrowing may have taken place via the Persian *qalambak*. It is not justifiable, however, to suppose that this word means lemon in Persian or Arabic; none of the lexicons identify it with that fruit.

To date, the most extensive philological treatment of the history of the lemon in China is that of the late Dr. Laufer, whose article in this journal has been referred to above. He says that the earliest reference to the lemon in Chinese works is in Fan Ch'eng-ta (writing A. D. 1175), who describes it as being of the size of a large plum and very sour. The name given to it here is *li-mung*. Chou K'ü-fei, in a notice of fruits cultivated in southern China in his time (1178), makes the same remarks, but adds that it was thought to have been introduced by the "southern barbarians." He also

<sup>32</sup> Duncan Forbes, *A Dictionary, Hindustani & English* (London, 1866).

<sup>33</sup> V. १२३.

<sup>34</sup> Watt, *op. cit.*, p. 325.

<sup>35</sup> *Op. cit.*, p. 243.

notes that the people of Canton used its juice as a substitute for vinegar and made the fruit itself into a preserve. Dr. Laufer's conclusion is that the form *li-mung* is phonetically too simple "and its congeners of almost identical structure are too widely diffused to afford a clue as to the particular nation or country from which the southern Chinese may have derived the fruit." His belief is that prior to the twelfth century the lemon must have migrated from India to Indo-China and possibly the Malay archipelago (and thence to China). But he admits that India is never referred to in Chinese sources in connection with the lemon; its culture was firmly established in the province of Canton in the twelfth century and neither at that nor at any later time do we hear of their importation into the country. The form *li-mu* of the Mongol period he believes to be based on the Persian *limū*; it is not listed in any of the dictionaries.

In 1933 there appeared an article by the Chinese botanist Shiu In-nin (more accurately, Hsiu Yu-ning)<sup>42</sup> in which many of Tanaka's assertions concerning the lemon were disputed. Tanaka<sup>43</sup> had said that the *li-mung* of the Sung dynasty records, to which he gives the name *Citrus limonia*, Osbeck, and which he regards as of the same species as the Otahite orange, the Khatta orange of India and *Citrus Volkameriana*, was distinct from the common European and American lemon, designated by him as *Citrus limon*, Burm. Tanaka pointed out that the former variety was broadly distributed in the Himalaya region, being adaptable to all sorts of climate. Hsiu questions the distinction made between these two varieties of lemon and believes the lemon to have been spread originally in a wild state throughout much of the Himalayan area, extending into China. He mentions a passage which says that in the fourth year of K'ai-pao (A. D. 971) two bottles of lemon juice were presented to the Emperor; this reference is thus 200 years older than the earliest given by Laufer. Moreover, it appears doubtful whether lemons were introduced into China from Malaya during the Sung dynasty, since from this reference lemons must have been popular in China at the very beginning of that period. Referring to the passage in Chou K'ü-fei he says, ". . . the fact that the people of P'anyu district, close to Canton, as also the people in wide areas

<sup>42</sup> "Lemons of Kwangtung with a Discussion Concerning their Origin," *Lingnan Science Journal*, XII, Supplement (Canton, 1933).

<sup>43</sup> Op. cit., p. 125.

surrounding, had been commonly using lemons prior to 1174 A. D. would make self-evident the fact that lemons were produced on a large scale even prior to this time . . . Under the circumstances it would seem unreasonable to believe that lemons were brought into Kwangtung from Malaya during the Sung dynasty; and if so, how would it have been possible for the distribution to have taken place on so large a scale? Moreover, how can one account for that period of time that should have elapsed before the people had worked out the various ways of preparing and preserving these fruits for food?" Regarding the wide distribution of the lemon, Hsin states that of about seventy references studied relating to Kwangtung, more than thirty have records concerning lemons. The distribution of lemons in Kwangtung, according to recent investigations, extends over sixty districts; moreover, in seven of these wild lemons are found. Hsin admits that some forms of lemons have been brought to Kwangtung for cultivation, but rightly adds that from this it does not follow that all lemons in Kwangtung are introduced. He quotes one Chen Ke-tsung as saying that *ming-meng*, *li-meng* and *li-mu* are not even known to exist in Malaya.

Regarding the "barbarous south" of Chou K'ü-fei, Hsin significantly points out the fact that cultural developments in Kwangtung came later than in Central China; during or before the T'ang (A. D. 618-906) and Sung (960-1273) dynasties scholars from the "barbarous south" are very rare, which accounts for the absence of literary references to the lemon prior to this period. To the supposition that the various ways of writing the Chinese word for lemon indicate its foreign origin, Hsin counters with the example of *Pearus* (*chi chü*), which is native to China and whose name is written also in the following ways: *chi kua tsu*, *mu chi chü*, *chian chia chi*, *pei shik*, *mu shik*, *pei hung tsu*, etc., etc. (p. 286). His conclusion (p. 290) is that the Chinese names for lemon have been in use over a long period of time throughout a great many localities. They should only be considered as general names for lemon as a group, but never for particular varieties or forms.

The evidence thus militates against Arab lands, India, and Malaya as being original centers from which the lemon was disseminated. In the province of Canton, on the other hand, not only are wild varieties of lemon found, but its cultivation is indicated at a period contemporary with the earliest Arabic notice. Not only was it merely cultivated, but its juice was preserved as a beverage;



this shows that by the tenth century the Chinese were already familiar with at least one method of preserving the lemon, and it is significant that such a use is not mentioned in Arabic sources until the time of *ibn-Jamī* (12th cent.). These considerations permit us to assume that the lemon had already been known in China for some time. In addition to this, there is the important fact that of all the Eastern Asiatic names for the lemon, the Cantonese *li-mung* most closely agrees with the Perso-Arabic *laymān*, *limān*; not only is the vocalism identical, but the Arabic preserves the nasal ending of the Cantonese word. There exists, it must be conceded, no absolute proof of the direct borrowing of the Arabic word from the Cantonese, but such is easily possible in view of the early Arab commercial intercourse with Canton, a fact which is well known. This is supported by words for lemon in lands bordering on China, which show borrowing from the Chinese. Annamese has *ninh-moug* <sup>44</sup> (cf. the Cantonese form *nīng-mung*); <sup>45</sup> Tibetan *li-men* or *li-mōn* *siu* is a transcription from the Chinese, <sup>46</sup> though the citron is called *gam-bu-ra*, from Skt. *gambhīra*. In view of this it is hard to understand why Dr. Laufer (*loc. cit.*) believes that "If the lemon is known to Tibetans, it must be due to importation from India, Kashmir, or Sikkim," unless he does so merely to uphold his belief in the Indian origin of the fruit. In Siamese the lemon is known as *lemōn* and *mandau*.<sup>47</sup> The first of these, if it is not a late loan-word from some European language, which would here most likely be English, can be derived only from the Chinese; *mandau* will be discussed presently.

From all the available evidence, the lemon seems to be a native of the Eastern Himalaya region, as both the geographical distribution and the various names of this fruit testify. Samuel Turner <sup>48</sup> mentions the lemon, as well as the lime and citron, under cultivation at Punakha, in Bhutan; he does not say what the native name was. Here, however, it appears to be an imported fruit (p. 140): "Punukka is esteemed the warmest part of Bootan, and, from its

<sup>44</sup> J. L. Taberd, *Dictionarium Latino-Anamiticum* (Serampore, 1838), s. v. *limon*.

<sup>45</sup> See Laufer, *op. cit.*, p. 153.

<sup>46</sup> *Idem*, n. 16.

<sup>47</sup> *English and Siamese Vocabulary* (Bangkok, Presbyterian Mission Press, 1865); Samuel J. Smith, *Anglo-Siamese Dictionary* (Bangkok, 1865).

<sup>48</sup> *An Account of an Embassy to the Court of the Tesho Lama in Tibet* (London, 1860), p. 138.



soil and situation, is chosen for the culture of exotics." *The Imperial Gazetteer of India* (new ed., XIX, Oxford, 1908, p. 47) mentions the luxuriant growth of lemons in Nepal, but whether the fruit here is native or introduced it does not say. The Nepali name, however (*nābu*, *nābu*), is of Indian type; the fruit may be a late introduction from India. The *Gazetteer* (XIX, p. 62) refers also to the cultivation of lemons in the Nicobar Islands, but here they seem to be introduced, and at a relatively recent date. Among the languages of Farther India and Burma,<sup>42</sup> Assamese *nāmā-tēngā* shows Indian influence; this is true also of Lhotā Nāgā *chāmō* (cf. Skt. *jambhā*), though this tongue belongs to the Tibeto-Burman group. Other Tibeto-Burman dialects have closely interrelated words for lemon, and these resemble neither the Indian nor the Chinese types: Kachāri *thāishāyān-shāgār*, Mikir *thēso*, and Rēngmā Nāgā *thāshāshā*. The early nineteenth-century American traveller Howard Malcolm<sup>43</sup> remarks that in Burma the lemon, which he calls *lieng-maw*<sup>44</sup> (this term is applied also to the orange, with the qualifier "sweet") is common and good, as is the lime, or *then-bā-yak*. In reply to my inquiry, Mr. Edwin H. Tuttle informs me that in his opinion the source of *then-bā-yak* is the Skt. *jambhāra*;<sup>45</sup> this raises the question as to whether the Tibetan *gam-bu-ra* does not mean lime, rather than citron, and whether the Lhotā Nāgā *chāmō* does not refer to the lime instead of the lemon. Though S. Kurz<sup>46</sup> says nothing of the lemon in Burma, White<sup>47</sup> reports it as growing wild on many mountain sides and upland plateaus. Another Burmese name for lemon, *sauk chān* (written *Arak khyān*, Chin lime) points to its spread from Upper Burma.

There are three definite series of words with which we have been

<sup>42</sup> See John Butler, "Vocabulary of Tribes of the Nāgā Hills," *Journal of the Asiatic Society of Bengal*, XLII, pt. 1 (1873), Appendix, p. xx; do., "Vocabulary of the Lhotā and Jalpōriā Nāgās," *JASB*, XLIV (1875), p. 233.

<sup>43</sup> *Travels in South-Eastern Asia*, 2nd ed. (Boston, 1839), I, p. 187.

<sup>44</sup> Written *liemā*, said to be pronounced *leimā* or *leimā*. (Tuttle).

<sup>45</sup> To quote Mr. Tuttle: "I presume it went thru Tamil, which regularly substitutes *s* for Sanskrit *j* and drops aspiration. The Tamil form would thus be *sambaram* or *sambaram*. . . . Burmese *θ* is historically *s*, and B. *r* has become *g*, so that your *sambaya* may represent a form \**sambara*, corresponding exactly to what I infer as a real or possible Tamil form."

<sup>46</sup> *Forest Flora of British Burma* (Calcutta, 1877), I, pp. 198-7.

<sup>47</sup> Herbert T. White, *Burma* (Cambridge, 1923), p. 84.

dealing. The first of these is the Indian type *nimbū* and its relatives; this word is a generic name for citrus fruits, but when used alone it means the lime.<sup>10</sup> The Chinese (Cantonese) type *li-mung*, *ming-mung* has given rise to Tibetan *li-mən*, *li-mən*, Annamese *ninh-mang*, Arabic *limān*, *laymān*, and (perhaps) Siamese lemon. These words all mean lemon and only that. Finally there is the type represented by the Burmese *limmā* and Siamese *manān* (with inversion of the first two consonants and change of *l* to *n*). To these is related the Old Japanese *liman* (see above, p. 11), given by Kern as the form from which those current in the East Indies are ultimately derived. In the East Indies the meaning is citrus fruits in general; in Burma *limmā* is used of both the orange and the lemon, from which its basic meaning *Citrus* appears.

In view of the dissimilarity of the Tibeto-Burman dialectical words reported by Butler from the Burmese and Cantonese equivalents, it seems very unlikely that the type *limmā*, *li-mung*, etc. is common Tibeto-Burman; this suspicion is strengthened by the fact that the Tibetan word itself is a loan from the Cantonese. The botanical and philological evidence discounts both India and Malaya as the home of the lemon, both word and thing; it favors, on the other hand, the eastern part of the Himalayas, whose foothills extend into both Upper Burma and southern China. From a geographical standpoint, too, this is the most likely place to look for the original habitat of the lemon, since from here it could easily spread to both India and Malaya, and thus to the islands of the East Indies.

Finally, the anthropological evidence points to the eastern Himalaya region as the focus for the spread of the lemon, both word and thing. S. M. Shirokogoroff, in his *Anthropology of Eastern China and Kwangtung Province* (Shanghai, 1925), p. 126 (Royal Asiatic Society, North China Branch, extra vol. IV), says: ". . . the coastal region of the present provinces of Chekiang, Fukien, and Kwangtung was occupied by aborigines who formed independent kingdoms and who were considered by the Chinese as an inferior race of barbarians. The annexation of these kingdoms took place in the third century before Christ, but the assimilation of their populations began, of course, much earlier. The present Kwangtung province, being a natural door to the Southern Sea, attracted the attention of the Chinese long before the conquest . . .

<sup>10</sup> Helen M. Johnson, op. cit., p. 50.

the [southern aborigines] were the same ethnical group as that of ancient Chekiang and the present Annamites. . . . At present there may be distinguished, besides the Kwangtung Chinese--Pentis—the groups known under the names of Hoides, immigrants of Fukien, who are still moving to the south (Hongkong, Singapore) and occupy the coastal region and that on the north-west from Canton . . . Ikie and Yao, not being numerous, are compared with Minotze and Burmans and inhabit the western region of Kwangtung." The Chinese (Cantonese) and Burman words for lemon then would seem to be derived from the Yao or some closely allied dialect; Chi Li (*The Formation of the Chinese People*, Cambridge, Mass., 1928, p. 255) puts Yao among the Mon-Khmer dialects still spoken in the province of Yunnan. However, I doubt whether there is any use spinning the discussion out so far as to trace the word for lemon down to the dialect where it originated, since there are too many unknown quantities to contend with.



## A NEW PHOENICIAN GRAMMAR \*

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THIS IS AN excellent work of reference, and its appearance is timely. Schröder's *Phönizische Sprache* served a very useful purpose in its day, but as a grammar it is long out of date and far from satisfying the present requirement. There is now available a large amount of new material, and its quality is even more significant than its quantity. The discoveries at Byblos and Ras Shamra, in particular, have given the Phoenician language a new aspect. Aside from the gain of material, advance in linguistic science has made a new treatment imperative. The manifold need is now met, and that very ably.

There is a well-marked literary language which can easily be made to cover all the monuments of Phoenicia proper, and whose later development in the western colonies can be traced to a considerable extent. To this material, scanty enough in itself, but given a satisfactory basis by comparison with Hebrew, and illuminated frequently from other Semitic languages, this grammar confines itself. Dr. Harris wisely resists the temptation to include the other Canaanite languages, or dialects, of which we happen to have some slight knowledge. Even the Ras Shamra material, which throws a most welcome light on some problems of Phoenician, must be kept separate. In the other direction also, it was well done to include only the best-attested and surely understood Neo-Punic material; for, in spite of the work of Chabot, in particular, we know as yet (and shall probably continue to know) very little in regard to this late branch of the language. In general, the classification and characterization here given of the various Phoenician dialects take full account of all the sources at present available.

It is an especial pleasure to see the phonology of the language adequately treated. In detail, there is of course much here which is still uncertain. The chapter dealing with morphology is equally

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\* *A Grammar of the Phoenician Language.* By ZEPH S. HARRIS. (American Oriental Series, Vol. 8.) New Haven, Conn.: AMERICAN ORIENTAL SOCIETY, 1920. xi + 172 pages.

well handled, and the very meager material, interpreted with caution, is given a firm framework. In all this there is very much to receive with gratitude and very little to criticize. In a multitude of places the reader would pause to express approval, or to raise a query, if time and space permitted.

Must not the Punic *y*, in the Latin transliteration, represent after all (see p. 5, note) a real modification of the vowel not capable of indication by the Latin alphabet? The credit for preserving the distinction could not be given to the manuscripts of the *Possidulus* (too much attention has perhaps been paid to them), but only to the tradition of the Roman stage, formed and confirmed through the presence in the Roman cities of many who knew exactly how the words should be pronounced. The resemblance to a familiar vowel-sound in Greek would explain the *y*, and the persistence of the stage tradition would account for the eventual insertion of this letter in some MSS. of Plautus; see Harris' remark at the end of his note.

Can it be maintained that 𐤓𐤕 and 𐤓𐤕 are *always* kept distinct in the Phoenician inscriptions? Nothing could be more certain than the eventual pronunciation of the former as a monosyllable, and occasional lapses from the conventional orthography would result. It is difficult to see in 𐤓𐤕 anything but the accusative particle in Bybl., line 2, "I invoke my Lady"; or in Eshmunazar, lines 9, 31, "May (the gods) utterly shut them out, the mighty prince or those men!"

The 𐤓 of the relative pronoun 𐤕𐤓 (p. 55) must be merely prothetic. This would indicate an earlier extensive use of 𐤕 as a proclitic with its vowel reduced; the phonetic development in either case being natural and regular.

Judging from the bit of material which we possess, the absolute infinitive had in Phoenician the same varied uses as in Hebrew, and was employed quite as extensively. We see it emphasizing its own finite verb, or developing the idea introduced by another verb; and at the close of the Ahiiram inscription we seem to have the coordination, familiar in Hebrew, of two such infinitives, a remarkably concise and effective idiom.

The supposed occurrence of *waw* consecutive with the perfect, namely in a certain group of three Punic inscriptions (see pp. 40, 43), seems unlikely; both because the use of this tense is otherwise unexampled in Phoenician, and also because the explanation

does not appear to be necessary. This may be a gnomic use of the perfect tense, for the verb in each case states what must then and there have long been customary and regarded as a matter of course.

The brevity of the chapter Syntax (11½ pages) tells its plain story. Inscriptions on stone or metal follow certain fixed types, and of Phoenician literature we know next to nothing. The Ras Shamra texts bring before us a fragment of one branch of the literature of a North-Canaanite people, fortunately preserved through the faithful instrumentality of clay tablets; and we have every reason to believe that at that same time there were similar texts, as well as elaborate works in other branches, all written on skins or papyrus, in Phoenicia proper. But this literature, like the others of Western Asia written on perishable material (excepting a chosen portion—a small fragment—of the Hebrew religious literature), was doomed to disappear utterly.

The earliest inscriptions which we possess testify plainly to the existence of literature in their time and region. Kilamu's boast (9th century), a rarely interesting document, is written with humor and the use of colloquialisms. Harris is inclined, with some hesitation (p. 65), to recognize a certain literary development, increasing freedom of expression, in the succession of monuments; but the nature of the material hardly warrants any such conclusion. The style of these brief carvings is conditioned by the chosen form and size of the monument, and by the amount of information which must be brought into very narrow limits, as in our modern telegrams. The "staccato style" of the Ahiram inscription was made necessary by its compass, two lines on the long rim of a sarcophagus. It is very true that "in the Eshmunazar inscription the style reaches the point of velubility"; but this long and twice-conceived document was composed (unless all the indications deceive) by an excited and justly apprehensive woman, who even appears to have changed her mind after the stone-carver had finished his first task.

More light is needed on the use of the definite article in Phoenician; see pp. 55 f., 66. The first commentators on the Ahiram inscription saw only a meager and uncertain employment, and were inclined to question its early use in Phoenicia. This judgment, based solely on comparison with Hebrew usage, was perhaps too hasty. No gain to the sense, in either clearness or force, results from repeating the article with the demonstrative pronoun; nor is it clear why the overloaded Hebrew idiom should be made the

norm of Canaanite speech. In the brief Ahiram text the use is logical, consistent, and in every way such as to satisfy the most captious grammarian who compares general Semitic usage. It was at that time a basic feature of the language. See also this *JOURNAL*, Vol. 45 (1925), 269 ff. As for Ras Shamra, it is just possible that if we possessed writings in prose we should find a definite article employed. Even in Hebrew poetry it is sparingly used (Ges.-Kautzsch, § 126, h), and some purely literary consideration might cause it to be omitted altogether.

It appears that Phoenician, like Hebrew, made occasional use of the accusative of place, as a variation from the ordinary construction with the preposition *ב*. Thus Eshmunazar, line 16, *ישכן אית עשתרת שם אורם*, "We gave Ashtart a habitation (caused her to dwell) in Shamem Addirim"; and the same is said of "Eshman, Holy Lord," in line 17, with the construction repeated. In Hebrew also, this accusative is now and then used with the verbs of dwelling or sojourning, *ישכן*, *ישב*, and *גור*. Another example is in the Bodashtart inscription CJS. I, 4, lines 4 f., *זכבן אית שרן ארץ ים ז*, "when he built our wall in this Sea District." These passages will be given further mention below.

In Chapter IV, Phoenician and the Canaanite Dialects, numerous keen and important observations are given in brief space. The encroachment of Aramaic on Phoenician, touched upon here, had already been mentioned on page 8. For obvious reasons, we know next to nothing about the use of Aramaic in Phoenicia in the Persian and Greek periods. For a long time there were Achaemenian government officials and garrisons in the chief cities, and later, Greek influence was very potent; both foreign languages must have been much used; but it is unlikely that the Phoenicians were ever bilingual to the degree that the Jews were from near the beginning of the Persian period onward.

The Phoenician Glossary, covering 83 pages, is a very valuable feature of the Grammar. It is a remarkably comprehensive and well-digested collection of the widely scattered material, and will be an indispensable help in all comparative studies which have to do with the Semitic vocabulary. This is followed by a list of the Phoenician inscriptions. In the mention of Bodashtart, p. 160, line 6, two different inscriptions are confused. The Bibliography, which fills ten pages, will be found most helpful.

The printing of the book is excellent, and the proof-reading admirably careful; the very few slips and misprints will be easily corrected by the reader. In the Hebrew type, it might be wished that *nun* and *gimel* resembled each other less closely.

The following foot-notes to the Glossary may be not unwelcome. It is possible to supplement here and there; and even in the most familiar royal inscriptions there are notorious difficulties, concerning both vocabulary and grammar, which are not likely to be discussed too often.

אנבעל. For the rare form אנבעל, a reference to this JOURNAL, Vol. 28 (1907), 354, might well be added, since the reading there is certain and the seal unquestionably genuine.

אגדר. Inclusion of this form of the name (Agadir, Cadix) is desirable, with at least a cross reference to גדר; both because of the frequency of its occurrence in the Phœnician coinage, and also because of its modern use. The accepted reading of the coin inscription is, I think, mistaken, for in the word hitherto read as מבעל (see the Glossary, בעל) the second letter is very distinctly פ, not ב, on all the coins alike. The true reading, I would suggest, is מפעל אגדר, "Arsenal of Agadir." This interpretation has history, as well as palaeography, as its support, for Agadir was the great naval and military base of the western Mediterranean, even long before the Punic wars. It was here that Hannibal equipped his armies for the invasion of Italy. *Mafal* is etymologically the perfect equivalent of "arsenal." The same word appears on the coins of Panormus in Sicily (another great naval base), as our Vocabulary also notes under בעל. This type of coin inscription appears elsewhere in colonial Phœnicia; thus מפקר לפקי on the coins of Leptis; see the Glossary.

There is a more primitive form of the inscription, פעלת הגדר, in Macdonald's *Greek Coins in the Hunterian Collection*, III, 664. Macdonald transliterates בעלת (see also p. 788), but his facsimile drawing shows quite unmistakably the letter פ. Add accordingly this word (— Heb. פִּעֻלָּה), as well as מפעל, to the Glossary under the root פעל.

אדר. The phrase שנת אדרם is wrongly connected here. It is not an epithet of Ashtart in Eshm., line 16, as line 17 conclusively shows. In both lines it is the name of the *mountain district* of "Greater Sidon," as is shown by the Bodashtart temple inscription (where the phrase is שנת רסם). See below, רשף and שמי.



חח. In the Ma'šab inscription we seem to have the dual of חח חח with the first member of the compound *uninflected*; that is, the compound is conceived as a single word.

חחח. The fourth line under this heading is to be cancelled; see under חחח, חחח, and חחח.

חחחח. Arsouf, the modern name, = Ἀρσούφια, the Greek name. (The equation has its bearing on Phoenician epigraphy, see below. The phonetic progression, Rēshéf, R'sháf, Arsháf, was of course inevitable.

חח. Heb. חח, "part, branch, member." The Phoco. vowel, heard as *o*, is always short, in spite of occasional *o* (as well as *eo*) in Grk. transcriptions. The pronunciation is hardly strange (see pp. 35, note 33, and 57, note 2), for in this proclitic word the labial consonant could give the vowel its color.

חחח. For the false reading חחחח, quoted from Macdonald, see above, חחחח.

חחח. The old but ill-founded conjecture, repeated by Lidzbarski and Cooke, in regard to the reading חחחח in Eshm., line 6, ought not to be perpetuated. The text reading is clear; the word, "their pratings, their foolish talk," is well attested (cf. especially Job 11:31), and precisely suits the context. For this, it is proposed to substitute a much weaker word, and to suppose that two of its letters were omitted by mistake. The fact, however, that the word is written alike in both copies of the text shows that the stone-cutter made no mistake.

חחח? (see חחח). The immediate context on either side of the word חחחח in Eshm., line 3, gives reason to doubt whether this is the same word which is familiar in the votive inscriptions. Cf. Ps. 90:5?

חח(ח)ח. The mark of interrogation must now be canceled; see the thorough discussion by Albright, "The Canaanite God Haurôn (Hôrôn)," in *AJSE*, 53 (1936), pp. 1-12.

חחח. Why may not the "first חחח" in the inscription be the passive participle? This yields perfect sense, and is grammatically sound: "rescued one, whom Pygmalion delivered"; there seems to be no other plausible way of treating the word. Passive participles of this stem are hardly to be found in Phoenician, though well attested in Punic; but see the Glossary s. v. חחח.

חחח. "Stripped off" does not suit the passage in the Ahiram

inscription. See p. 43, middle, where a better rendering is suggested.

יגל. In the two proper names, the first element, instead of "fear," may perhaps be יגל, "pay, reward"; a verb which probably had a wider use in Canaanite speech (even without Aramaic influence) than the Hebrew lexicon would indicate.

ידל. Scholars dealing with Phœnician inscriptions, students of the antiquities of Sidon, and authors of textbooks, all seem to be unaware of the important fact that we know the exact location of the fountain which in Eshm., line 17, is called ידל יע. The temple in the mountain district of Sidon, dedicated "to Eshman, Holy Lord," has been partially excavated; its building stones bear the title by which it is designated in the passage just named. A very noticeable feature in its immediate neighborhood is a boiling spring which bursts forth for an instant in a strong stream and then as suddenly disappears underground, eventually watering the gardens of Sidon. This, beyond question, is the יע bearing the problematic name.<sup>1</sup> The second element in the name is a verb; perhaps descriptive? See this JOURNAL, Vol. 23 (1902), pp. 167 f.

יש. Root conjectured in the Ahiram inscr.; see the following note.

ז. This in Phœnician represents both the particle of comparison and the conjunction (Heb. ז). Ordinarily there can be no doubt which of the two was intended, but the first entry under the meaning "as" in the Glossary is very questionable. נשמה is here rendered, with a query, "as his resting place." The derivation of שמה (שנת) from יש is ingenious, and Albright's name behind the conjecture carries weight; but "sleep" and "sleeping place" are two quite different conceptions. The particle of comparison seems out of place; and "for his rest" could not be expressed by the particle ז. It would rather seem that the latter must here mean

<sup>1</sup> When I saw this fountain, in 1901, it seemed to me to emerge in an orifice designed (for the benefit of the temple) to give access to the water of an aqueduct. Maeridy Bey, who later in the year made the partial excavation of the temple, discovered also, a little higher up on the mountain, the remains of a large well, or cistern. Concerning the aqueduct he wrote (*Le Temple d'Eshmoun à Sidon*, 1904, pp. 37 f.): "Le canal est celui que M. Gaillardet regardait comme phénicien, et tout porte à croire que cette opinion est solidement établie. En tout cas, ce même canal sert aujourd'hui à alimenter Saïda, et mériterait une étude plus approfondie."

"when"; that is, it is the equivalent of Heb. **כִּי** in one of its very common uses. In view of the regular employment of **שָׁנָב** for the sleep of death, and the very frequent use of the verb **שָׁחַת**, in these documents, it seems much more probable that the phrase should be rendered "when I (or they) laid him away." Cancel, accordingly, the entry **יָשָׁן**.

In the Bodashtart wall (?) inscription (CIS I, 4), line 4, **בָּנָב** certainly means "when he built." I at one time thought of connecting the particle with Heb. **בָּנָה** (Glossary, p. 108, below), but I am now quite convinced that in all such cases it is the Heb. **כִּי**.

**בָּנָב**. This, on the well-known Sidonian coin of the 2nd century B. C., certainly stands for Carthage, but the origin of the name is a puzzle. The occurrence (rare) of the variant **כָּכָב** (*Kakkabā*) on these same coins is very remarkable. See especially the facsimile in Babelon, *Les Perses Achéménides*, Pl. XXX, 20.

**יָלָן**. This root should not be booked, without the mark of interrogation, for Kilamu, line 10. The question is between the readings **יָלָן** and **יָלָן**, that is, between "they went about like dogs" and "they twisted, writhed (Arab. *talawwā*) like dogs," as the description of the inferior party in the kingdom. The form of the penultimate letter of the word is puzzling. As it stands, it is neither *waw* nor *kaf*; it has neither the shaft of the former nor the head of the latter. Lidzbarski (*Deutsche Literaturzeitung*, 1912, p. 96; *Ephemeris*, III, 233) chose **כ**, on the ground that the long shaft is curved to the left, quite unlike the shaft of **ל**. In view of what Von Luschan has said in regard to the badly damaged condition of the raised letters of the inscription (*Ausgrabungen in Sendschirli*, IV, 374 f.), it is worthy of notice that the presence of a very small fragment of stone on the chin of this letter would make it the exact counterpart of the other examples of **כ** in the inscription. This reading seems, on all accounts, to deserve the preference, even if no certainty can be claimed.

**לָכָן**. Lidzbarski's conjecture of this word in Kilamu, line 6, encounters several difficulties. It goes against the word-division of the inscription, and results in a clause which is grammatically difficult as well as unsatisfactory in its meaning. See below, under **לָכָן**.

**לָכָן**. Probably very few who have read Ronzevalle's *L'Alphabet du Sarcophage d'Abiram* (Beyrouth, 1927), especially pp. 11, 22,

36-38, would doubt that the inscription ended with the words **שכפל שפל**; and the clear letters in the photographic facsimile at the bottom of his Plate III put the matter beyond any doubt whatever. The two infinitives, with their uniform vowel-pointing, give the document a close which is high-sounding as well as suited to the sense. On the meaning of the final word (*shafal* of **שכפל**) and of its association with **שפל**, see this JOURNAL, Vol. 45 (1925), p. 274. Cancel accordingly, in the Glossary, the form **שכפל** under root **שכל**.

**סן** "precious object." In Tabnith, line 5, the reading **סנכ** (plur.), all but universally adopted (because of its occurrence in Eshm., line 5, where it is in place?), is grammatically objectionable. In this emphatic negation, **כ** must be followed by the singular number, and the true division of the words is **סן סכסר**. For the meaning of the (*pu'al*?) participle, cf. Assy. *mašāru*, "leave"? "There is left here with me neither gold nor silver nor any valuable thing." Cf. Nerab, II, line 6. Cancel **ש** in the Glossary.

**שכל**. Why should the participle be questioned in Eshm., line 9? it would seem the natural form to use. Cf. also 1 Kings 18: 8 and 21: 11. It is the form used in the Bodashtert temple inscription, governing **סן** **שכל**, etc. Lidzbarski's reading of the latter inscription is thoroughly mistaken; see below, under **שכל**, **שכל**, and **שכל**.

**לל**. There is here a gap in the Glossary which it is important to fill. The jussive, **לל** **לל**, "may he not obtain!" occurs in Tabnith, line 7, though it has been generally unrecognized. The verb is very common in Arabic, and there are certain traces of its use in Aramaic; see ZA, Vol. 26, pp. 80-83. It has been customary to emend the text thus: **לל** **לל** **לל**, as though it were conceivable that in this brief and very carefully executed royal inscription the stone-cutter could accidentally omit the rather conspicuous letter **ל** twice in a series of five letters! Equally important, the second person is not in place here at all. *The imprecation is regularly expressed in the third person, in both Phœnician and Aramaic inscriptions.* (An apparent exception is Nerab I; but here the curse is followed by a blessing, also in the 2nd person.) Even if the 2nd person has just been used, nevertheless when the words of the curse are reached there must be a change to the third person. Thus in Yehawmilk (Byblos), 2nd pers. in line 13, but 3rd pers. in line 15. Nerab II, 8-10: "Whoever thou art that shall injure me . . . may his posterity perish!" The Guzneh Boundary Stone,

published by Montgomery in this JOURNAL, 28 (1907), pp. 164 ff.: "Whoever thou art that shall destroy . . ., the gods will destroy him and his seed." Notice also the similar change of the persons in the *Syr.* inscription in Lidzbarski, *Handbuch*, p. 484, no. 9. The verb in Tabnith is an important example of the fact that not all Phoenician roots are to be found in the Hebrew dictionary.

סנר. In the Eshmun. inser. this verb can hardly mean "deliver up"; the signification is not really plausible in line 9, and in 21 f. it would be very far-fetched. The verb more probably means "cut off," "make an end of," practically synonymous with the following לקצתנם, as lines 21 f. seem to show. The נא is the accusative particle, see above. The phrase in the second half of line 9, in this emotionally conceived document, is not logically complete, for it omits mention of the private citizen. It seems plain that all the thought of Amashtart, who composed the inscription, was on "the mighty king who rules over them," probably the immediate successor of Eshmunazar II, who was expected to make the attempt to remove the sarcophagus from the royal necropolis (and in fact it does seem to have been thus removed!).

עשתרת שטם אדרם. See above, on אדר, and cancel this entry in the Glossary.

ןצ. I believe that renewed examination will show with certainty that the designation of locality, יאן צן (ןצ), occurs not only in Eshmun., line 16, but also in the Bodashtart wall (?) inscription, CIS, I, 4. The fact is unrecognized by the editors of the *Corpus*, also by Lidzbarski, Cooke, Clermont Ganneau, and the *Répertoire*. The *pod* of יא (') is broken off the corner of the stone, but the traces of the *mem* can be seen in the CIS heliograph, and are even plainer on the stone itself, in the Louvre. The note in the *Corpus*, p. 22, reads: "Levy et Schlotmann: יא, quod vestigia in lapide adhuc remanentia non permittunt." The reading, that is, had been the subject of controversy, and was decided by the supposed meaning of certain words. The "vestigia" of יא seem quite unmistakable; while measurement, with due regard to the alignment, shows plenty of room for the '. See, for details, this JOURNAL, Vol. 28 (1902), p. 171. This reading disposes of Lidzbarski's rather fanciful conjecture, יאן צארת, "Sharon is our Land," as the name of a temple (*Epé.* II, 53). The meaning of יאן is still unknown, beyond the fact that it designates something which was "built." The interpretation "our wall" is certainly a

possibility, and it seems to be given support by the Bodashtart temple inscription; see below, under שרר. The supposed "names of two temples" given here in the Glossary, שר צדן and שר צדן כשל, are the result of misunderstanding, as will presently be shown.

צדני. The rendering "Sidonians" for the צדני of the coins and inscriptions may be strongly doubted. There is no other example of a Phœnician city coining in Phœnician characters in the name of its citizens, after the Greek fashion; and the meaning is sometimes out of place, as when "Sidonians" are said to be "the mother of Carthage, Hippo, Citium, and Tyre." Similarly, on the boasting coin of Tyre, צדן אמ צדן must be rendered, "Of Tyre, the mother of Sidon." In the bilingual coinage of the Phœnician cities, the customary Greek legends are *Αραδίων, Βυβλίαν, Μαράθηναν, Τραπεζοῦσαν, Τύρον*, etc., but the Semitic is always the name of the city, ארד, ארד, ארד, etc. As for Sidon, there is good reason to believe that the plural was adopted locally and at a rather late date, in the pride and rivalry, otherwise attested, of the great city. The צדן רב of Josh. 8:11 and 10:28 comes from the same time. Justification for this term, "Greater Sidon," may be found in the three great divisions, Sea district, Reshaf land, and High Heavens, which are named in the Bodasht. and Eshm. inscriptions. In the former the three are named as "belonging to Sidon" (צדן; the original name naturally used here); and in Bodashtart's wall (?) inscription the singular number is necessarily used, as applying to a single district, the "Sea land." צדן occurs occasionally on coins.

צדק. The reading צדק בן, and the rendering "legitimate heir," should be furnished with question marks. The shorter building inscription of Bodashtart has not yet been given convincing interpretation. Clermont Ganneau conjectured "legitimate off-spring" as the meaning of צדק צדק in the Narnaku inscription, but can the context really permit this?

צדק. See the preceding note.

קדש. The phrase קדש קדש in the Eshm. and Bodasht. inscrs. cannot mean "prince of the shrine," for the article would have been used, as in Lidzbarski, 97, and as it is in even the Neo-Punic texts. The true rendering is "holy Lord," the adjective corresponding to Heb. קדש.

קנא. The Syriac *gnōm* is a compound made up from *gnō* and the indefinite *m(a)*, like *bram*, *klām*, *meddēm*, etc. There seems

to be no ground for carrying over this Aramaic formation into Phoenician. The meaning "persona" is the fruit of a late development in Syr., where the older use of the word seems to have been merely emphatic. The component elements of Phoen. קננני are therefore קנני, not קננני. The origin of the monosyllable קננ is a problem; perhaps it is the very widely and variously employed Persian *gōa* (color, fashion, manner, nature, essence), which seems to occur in Dan. 7:15 בְּגִין רְנָה, "by reason of this" (LXX, ἐν αἰτίας). This could account for both the Syriac and the Phoenician use, the borrowed word serving, in either case, to give emphasis to an indefinite pronoun.

קננני. Lidzbarski's reading of רשפם (plur.) in the Bodashtart temple inscription is certainly mistaken, the כ is indispensable as the initial letter of the next-following word. ארץ רשף was the name of one of the parts of "greater Sidon," and the persistence to the present day of the name "Apollo" in the SE. plain assures the location. See this JOURNAL, Vol. 29 (1908), 193 f.; also the note above, on ארשף. Bodashtart describes himself as "reigning over" (בשלת-כ) the three districts: ארץ רשף, ארץ כנען, ארץ כנען. The clause was read correctly in the first publication of the inscription, in this JOURNAL, Vol. 23 (1903), pp. 159 ff.

ש-ש. In a fragmentary text, a reading that makes good sense, and also makes possible a plausible restoration, is to be preferred to one that makes no sense at all, even if certainty is not attainable. Read שש, not שש, in the Yehawmilk (Byblos) inscr., lines 12 and 13; and both lines can now be restored. See ZA, 26, p. 78, and cf. also the words of Gudea in Barton, *Royal Inscriptions of Sumer and Akkad*, p. 201.

ש-ש. Add שתה, 3 pl. or 1 sg., w. suffix, Ahiram 1; see note above, on שש.

שלה. Cancel the Kilamu passage as an example of the verb. Lidzbarski's rendering supposes an improbable use of כל, a less usual stem of the (conjectured) verb, and such omission of the verb's object as leaves the clause without force or even clear meaning; besides going contrary to the word-division which the inscription supplies! By reading the noun, שלה, and the verb as ש(ה)לל, a clause is gained which exactly suits the context: "every weapon humbled them"; cf. Judg. 6:6!

שם. Add to the Glossary the adverb שם, "there," and see the note above.

שם. On the false connection of the name Ashtart, see above, under רר. The suburb of Siden called שם רם ran along the slope of the Lebanon range, which here rises evenly and rather abruptly from the coastal plain. It was a narrow strip, about two miles in length, running clear to the Awrāly (Bostrenus) river, as we know from the two royal inscriptions and the partially excavated temple of Eshmun. The name seems to belong to Phoenician mythology, for Sanchuniathon's Σαμμερμας (which Harris treats with his usual caution) must stand in some relation to it. The strip of hillside was crowned with temples, two of which are mentioned in our sources. The sanctuary (now a Muslim shrine) which lies just above the modern city may be the site of still another of the ancient temples. The names of the three districts were presumably of merely local use, and they appear in slightly varied form: שם רם and שם רם, שם רם and שם רם. Only one of them, the "Reshef land," has left its trace in modern times.

שר. This, in CIS 4, 4, is certainly not the word "plain" (plains are not built), nor is it part of the name of a temple (*Epheueris* II, 53); see above, note on רר, where the rendering "our wall" is suggested; also the note which here follows.

שר. In the Eshmun temple inser., שר cannot be rendered "Siden prince" (Lidsbarski, and before him Berger), for "Siden" is always feminine, and "princess" would be שרה (Heb. שרה). This, however, is not the main difficulty with the rendering; much more important is the requirement of the context. Bodashtart has finished giving his titles, and now comes to the mention of his achievements. He says of himself: שר בן אש, words which must form a complete clause, since they are immediately followed by the sentence: "This house he built to Eshmun, Holy Lord." שר בן אש is of course, "He who built (buildings)"; the verb used without expressed object, as frequently in Heb. The remaining letters seem certain throughout, and they are given uniformly<sup>2</sup> in the several copies of the inscription; what is to be made of them,

<sup>2</sup> There are two exceptions: שר, with ש, is the reading of the inser. photographed in Tafel IV, in Von Landau, *Vorläufige Nachrichten über die im Eshmuntempel bei Siden gefundenen phönizischen Altertümer* (Berlin, 1904); and this, though with a differently made ש, seemed to be the case also on the stone (now in the Louvre) published by the present writer. Again, in Von Landau's Tafel III the reading is שר (Ašr) (see below):



is not at first clear. If **רש** may be regarded as a denominative verb from *skār*, "wall," and be given connection with **שרן**, which Bodashtart "built," according to CIS 4, in **ימ ים** ("our wall in this Sea district"), the clause may be rendered: "He who was a builder, and walled Sidon" (*viz.* Sidon-yam). Observe also, in the preceding footnote, the mention of a variant reading **רש**, though this is probably nothing more than a stone-cutter's blunder.

**תבנית**. A reference might well have been added, to Heb. **תבנית**, "image, likeness," which is both from root **בני** and a suitable element in a proper name.



## BRIEF COMMUNICATIONS

### *The history of Tamil R*

THE SOUND of the seventeenth consonant of the Tamil alphabet is commonly called a "strong *r*." I use *R* to transliterate this Tamil letter, and assume voiceless *r* as its ancient value. Recently L. V. Ramaswami Aiyar has published an essay on the sounds of *R*, in two numbers of the 8th volume of the *Journal of the Madras University*, entitled "The history of the Tamil-Malayalam alveolar plosive." Ramaswami Aiyar is the author of numerous papers on Dravidian linguistics, and he has kindly sent me a large number of his writings. Some of these are beyond me because they require, for their proper understanding, a greater knowledge than I possess of Kēlan (Malayālam), his native tongue. But where I could understand his writings, I have sometimes gotten from them an impression of shallow scholarship. He seems to understand the general principles of linguistics as enunciated by Bragmann's school; but he does not seem to have kept in mind that these principles should be applied to the study of Dravidian linguistics.

Ramaswami Aiyar's paper on *R* is valuable in that it tells us definitely the present values of this letter in Tamil. Intervocalic *R* has become a voiced sound and is said to be slightly reverted, as compared with ordinary *r*. Before a voiceless consonant, it has either the same sound or that of alveolar *t*. Corresponding to written *RR*, spoken Tamil has various values in different regions: dental *tt*, alveolar *tt*, reverted *tt*, alveolar *tt̪r*. Corresponding to written *nR*, spoken Tamil has alveolar *n̪r*, with the variants alveolar *n̪*, alveolar *nn*, reverted *np*.

In Chatterji's works on Bangālī, *t* and *d* are used for dentals, but *n* stands for alveolar *n̪*, because that is the regular value of *n* (except in contact with a dental) in Bangālī. Ramaswami Aiyar, apparently failing to understand the reason for the special value of *n* in Bangālī, has undertaken to follow Chatterji by using *t* and *d* for dentals and *n* for an alveolar in transcribing Dravidian, while he uses *t'* and *d'* for alveolars and *n'* for a dental. This crisscross arrangement is hard to keep in mind and shows that the author does not understand properly the principles of fonetic transcription.

The author seems to have found it difficult to remember his own system. Thus on page 1 he tells us that an initial *n* is always dental in Kēlan; but we find that he uses unmarked initial *n*, the symbol of the alveolar sound, in transcribing various Kēlan words: p. 10, p. 15, p. 21 (twice), p. 27, p. 30, p. 31 (eight times). On pages 22, 24, 25, Telugu *-śūṭamula* is transcribed with unmarked *nt*, implying—wrongly—an alveolar *n* and dental *t*; evidently the whole group *nt* is dental.

The author gives lists of words containing *R* in Tamil, with cognates in other Dravidian tongues; these words are divided into sixteen categories. As general rules he gives the equations Tamil *RR* = Kēlan alveolar *tt* (written as *RR*) — Kanara *tt* (reduced to *t* after a long vowel) = Telugu *t* (reduced to *ṭ* after a long vowel) = Tulu *tt* (reduced to *t* after a long vowel); and Tamil *nR* = Kēlan dental *nn* — Kanara *nd* = Telugu *nḍ* = Tulu *nd(ḥ)* — Kui *ndā*. Here the author overlooks an important principle of Kanara phonology. As I have pointed out in *Dravidian Developments* (cp. also JAOS 57. 307), Kanara regularly loses a checking nasal after a long or weak-stress vowel. Hence we find Tamil *nR* — Kanara *R* in *mānRu* = *māRu* (three). From what the author says about *mānRu* and *māRu* in the second portion of his essay, it seems that he does not understand this obvious principle of Kanara phonology.

In category 1 the author gives *panRi* (pig) — Kēlan *pannā* = Kanara *panḍi* — Telugu blank — Tulu *panḍi* — Kui *panḍi* = Gōndi *panḍi*. The Kēlan form should have short *i*. The Kanara form is wrongly given with alveolar *n* instead of dental *n*. The Gōndi form should have long *i*. From the Telugu blank, most readers would get the idea that Telugu lacks a cognate corresponding to Tamil *panRi*. This idea would be wrong, for Telugu has the form *panḍi*. It might be supposed that the Telugu word was left out by accident. But the author shows us that such a supposition is wrong too. On p. 16 he gives the same equation again. Here he gives correctly the Kēlan, Kanara, and Gōndi forms, but the Telugu column is again blank. The omission was therefore intentional. Why did the author leave out the Telugu form? Evidently because it did not fit his rule. According to his rule, Telugu should have *\*panḍi* instead of *panḍi*. This illustrates what I call the author's shallow scholarship: a word does not agree with his rule, so he leaves it out, giving a wrong impression as to the existence of the word. Instead of leaving it out, he ought to have

put it in and tried to explain it. A simple explanation would be that Telugu *pandi* is a form borrowed from Kanara.

It should be noted that Kui *padāi* does not agree with the author's general rule: Kui *nāi* — Tamil *nāi*. From the agreement with Gōndi *padāi* in regard to the lack of a nasal, it is most likely that *padāi* is normal. The difficulty lies in the fact that we do not know the source of *pāuḍāi*. If it is connected with *pal* (tooth) and represents 'palis, we may assume *panḍi* < \**pantri* < \**pandri* < \**paldri* and *padāi* < \**paldāi* < \**paldri*.

The author's 4th category is made up of words meaning "small" or "short": Tamil *kaḷu*, *ciḷu*, Kanara *kuḷu*, *kiḷu*, Telugu *kuḷu*, *ciḷu*. The author fails to discuss the source of these words. To me it seems fairly clear that their source was Sanskrit *ḥṛudra*, with a general Dravidian development *R* < *tr* < *dr*; the *i*-variants can be explained by blending with some Dravidian word containing *i*, such as *ciṇna* "small."

Under category 5 the author deals with a rule of Tamil sandhi: *i* + *i* makes *RR*, or sometimes (after a long vowel) *R*; the variant *hR* is found in early Tamil. The author tells us that this *hR* "has been dealt with by me elsewhere," but he fails to state here what conclusion he reached about the *h*. My inference is that a nominative-ending *-s* was involved in the development. From Brāhmi *pāḷ* (milk) corresponding to Tamil *pāl*, I have inferred a nominative \**pāls*, with *ḷ* (voiceless reverted *l*) from *ls* in Brāhmi. Similarly from Tamil *kaḷḷitu* beside *kaRRitu*, from *kal* (stone) and *tita* (bad), I infer early Tamil \**kaḷs tita*, with *hR* < *str* < *lst* as a variant of *RR* < *tr* < *it* < *lst*.

Under category 6 the author takes up another rule of sandhi: Tamil *l* becomes *R* in contact with *p*, *c*, *k*. To my mind this development proves clearly that voiceless *r* was the ancient value of *R*.

Under category 7 (misprinted "9") the author discusses Telugu *vāḍu* (he) and its cognates. He fails to make out their history. As I have shown (JAOS 57. 114), the developments involve the use of *vāḍu* (being) as a nominative-ending: Telugu *vāḍu* < \**avāḍu* < \**avāḍru* < \**asavāḍu* < \**asavāḍru*, Kui *avāḍu* < \**avāḍru* < \**asavāḍru*, Gōndi *ōr* < \**avāḍru* < \**asavāḍru* < \**asavāḍru*.

In a paper on "Dravidian Nominal Inflections" printed in the *Educational Review* of Madras for September, 1936, Ramaswami

Aiyar says: "For the *s* of Kurukh *as* [1] 'he,' I can only suggest that it looks like a development of *na*' such as the one that appears in Telugu inscriptional *vānd'u*." It is strange that the author has overlooked the correspondence of Tamil *avan* (there) and Kurukh *asan* (there). Tamil *avan* (he) and Telugu *vān-* (nom. *vāṇu* < *vāṇḍu*) evidently have the same root as *avan* (there). Kurukh *asan* evidently has the same root as *ās* (he). The developments were Tamil *avan* < *\*aan* < *\*asan* and *avan* < *\*aav* < *\*aav*; Kurukh *asan* < *\*asav* and *ās* < *\*asa* < *asan*.

Under category 8 the author deals with the Tamil neuter plural *avai* (they), having the inflectional stem *avaRR-*. It appears that he does not know the history of these forms. Tamil *avai* represents *\*avas*, an accusative used for the nominative and therefore not used as the general stem. The stem *avaRR-* represents *\*avastr-* < *\*avastī-* < *\*avastī-* < *\*avastakā*, and is thus a doublet of its variant *avastakā* < *\*avastakā*; *-kai* is the regular plural-ending of neuter nouns.

Under category 10 the author discusses Dravidian "one" and "three" without any consideration of "two." With regard to Tamil "one" he says: "That there is relationship between *or* and *ond'r* is of course beyond all doubt, but the nature of this relationship is not sufficiently clear." It becomes clear by comparison with the forms of "two." Tamil has *oru*, *ōr*, *onRu* (one), *iru*, *ēr*, *iraṇḍu* (two), *mu*, *mū*, *mūnRu* (three). Brāhmi *ira* seems to imply *\*rā* as the basic form of "two." Neuter nouns were formed with the use of *uṇu* (being) as a suffix. The development was *onRu* < *\*onru* (or *\*onru*) < *\*oruṇu*; *iraṇḍu* < *\*rāṇḍu* < *\*rāṇḍu*; *mūnRu* < *\*mūnru* (or *\*mūnru*) < *\*mūruṇu*. The *r* of this last was taken from *iraṇḍu*; otherwise the form would have been *\*mūruṇḍu*.

Under category 16 the author includes Kanara *aṣu*, *aṣu*, *āṣu*, *aṣu*, *āṣu*, *aṣu* (so much). His ideas of the history of these forms seem all wrong. He assumes that *āṣu* came from *\*attū*; that the *ṭ*-forms were taken from Telugu; and that *aṣu* is a blend of *āṣu* and *aṣu*. He says nothing about *aṣu*. Kanara *s* always stands for an older *c* in native words: *āṣu* is from *\*āc*, presumably from older *\*ac*; *aṣu* represents *\*actū*, from the same *\*ac* with the neuter-ending *-tū*; the other *ṭ*-forms seem to be dialectal variants of *aṣu*. I take *aṣu* to be a dialectal variant of *āṣu*: cp. French *plaisir* beside Provencal *plazer* < *placere*.

The author concludes from his comparative study that the earliest value of Tamil *R* was an alveolar occlusive. I do not agree with this. As I have stated above, the development of *R* from *l* before voiceless consonants points to voiceless *r* as the early value of this letter. Evidently *l* became voiceless in contact with a voiceless sound, and then changed to voiceless *r* because the latter sound already existed in the language, for example as the derivative of *rs*; cp. Brāhmi voiceless *L* derived from *rs* (JAOS 56. 358). The author says (p. 42) that from the earliest known times Tamil grammarians have treated *R* as a "plosive or [member of the] *vallīnam*." I cannot admit this interpretation of *vallīnam*. Tamil *val* or *valla* means "strong"; *vallīnam* is the class (*inam*) of strong sounds, that is, voiceless ones.

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### *The "Four Causes" in the Bhagavad Gītā*

In *Bṛhadāraṇyaka Upaniṣad*, IV. 4. 2, the event or act of coming to birth is referred to two immediate causes, knowledge and action (*vidyākarmṇi*, in which compound *karmṇ* represents the causative power of past acts, and *vidyā* a first cause or Providence, *pūrva-  
prajñā*). It is interesting to compare with this Bhagavad Gītā XVIII. 14-15, where events in general are explained by means of four immediate and one first cause. The passage runs "The causes (*hetavaḥ*) of whatever it may be that a man undertakes (*prā-  
bhate*, cf. *samanudrabhate* in the BU. context), whether physically, verbally, or mentally, and whether ordinate or inordinate, are the 'standing-ground' (*adhiṣṭhānam*), the 'maker' or 'doer' (*kartā*), the various 'motive' (*karaṇam*,—reason, occasion, wherefore), and multifarious 'motions' (*caṣṭāḥ*), and therewith as fifth the Divine" (*daivam*, which Barnett admirably renders by "Providence").

It will be seen that the four immediate causes thus predicated coincide with the four Aristotelian causes, *adhiṣṭhānam* corresponding to the material, *kartā* to the formal, *karaṇam* to the final, and *caṣṭāḥ* to the efficient cause, added to which is the *daivam* or Providential cause, corresponding to the *pūrvaprajñā* of the BU. context. It should be noted that *ky* being as much to "make" as to "do," the explanation applies with equal validity to *factibilia* and *agibilia*: the *kartā*, whether as maker (artist) or doer, being

either way the formal cause, because it is in him that there subsists the idea of the thing to be made or done, in an imitable form, whether as "art in the artist" or as "prudence in the deer," respectively the *recta ratio factibilium* and *recta ratio agibilium*.

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#### *A Note on the names Armānum and Urartu*

SINCE THE early stages in the decipherment of cuneiform it has been known that the sign denoting Akkad (*uri*) was also the sign used by the Assyrians of the 1st millennium B. C. to denote the kingdom of Urartu (*tilla*).<sup>1</sup> In *CT* 11, 15, 2, 5 the sign, in the sense of Akkad, is read *uri* and in *CT* 11, 15, 2, 7, in the sense of Urartu, it is read *tilla* (see Deimel, *Sumerisches Lexikon* 359).<sup>2</sup> This strange double rendering has remained a puzzle, toward a solution of which many scholars have made contributions.

Now the northern campaigns of Nārām-Sin in the first half of the 3rd millennium B. C. have been the subject of much speculation. This great king has become, in a sense, the "mystery king." Very soon after his death he had become a figure of legend; cf. Smith, *Early History of Assyria*, p. 94 ff. and Gelb, *Oriental Institute Publications* XXVII (1935), pp. 5-6. We know that Nārām-Sin was called the conqueror of the lands of Armānum and Ibla (for references conveniently listed see Gelb, loc. cit.). Ibla, it is now generally accepted, lay somewhere in Syria, north of Iarmuti.<sup>3</sup> On the location of Armānum opinion has been varied. Sidney Smith,

<sup>1</sup> This later usage is particularly noticeable in the Assyrian letters; cf. Streck, *Zeitschrift für Assyriologie* 14 (1899), p. 194. Whether any significance can be attached to this fact it is, at present, impossible to say.

<sup>2</sup> Cf., for further references and comments, my dissertation: "The Kingdom of Van (Urartu): Its Origins in Relation to the Hurrian Problem" (Harvard, 1930; unpublished), p. 196 et passim. I wish to acknowledge here my great indebtedness for help with special phases of this paper to Professors R. H. Pfeiffer and E. A. Speiser, who of course are not responsible for whatever errors it may contain.

<sup>3</sup> See, e. g. Maister, *Untersuchungen zur alten Geschichte und Ethnographie Syriens und Palästinas I* (1930), p. 7 ff. and Unger, *Forschungen und Fortschritte* 10 (1934), p. 231.

*Ur: Royal Inscriptions—Text* (— URI), pp. 80-81, followed by Gellb, loc. cit., identified it with Assyrian Halman or Halpi (Aleppo), whereas Albright, JEA 7 (1921): 80, n. 1, among others, was inclined to equate it with the later Armenia. This latter opinion I think is correct now that the exact place of origin of the famous Diarbekr Stele of Narâm-Sin is known. For the lower, hitherto missing, portion of the stele has been found, so that we know the spot on which Narâm-Sin erected it. This Diarbekr Stele was erected at Pir Hüseyin, a few miles N. E. of Diarbekr, on the Ambar Chai.\* Dr. Naub, with long study and residence at Pir Hüseyin, also reports the existence of numerous tells in the immediate neighbourhood from a surface examination of which it would seem that the site was one of considerable importance. This fact strongly suggests that here may have been the Akkadian "colony" predicted by King, loc. cit. Cf. H. C. Rawlinson, *Herodotus* (1893) IV, p. 207 ff.

Furthermore, it would seem that the region around Pir Hüseyin might be the Armânum of URI 275 cols. I, lines 7, 13; II, line 4; III, lines 4, 24(?) and 30.<sup>2</sup> For the order of the geographical territories listed as conquered by Narâm-Sin would seem to show that he progressed from east to west, thus making Armânum lie east of Ibla, whose location is reasonably certain. It is interesting to note in this connection that this region is also, roughly speaking, where the unknown writer of the geographical commentary on the campaigns of Sargon of Akkad placed Armânum (*KAVI* No. 93, obv. l. 19), making it lie next to Lullubu and Akkad. At least from the context this would seem to be so. Cf. Albright, *JAOS* 45 (1925): 193 ff., esp. 212 ff., but see Speiser, *Mesopotamian Origins*, p. 88 ff.<sup>3</sup>

\* See now J. P. Naub and E. Unger, "Die Entdeckung der Stele des Narâm-Sin in Pir Hüseyin (mit 10 Abb.);" *Istanbul Asyriatika Neşriyatı* XII. King, *History of Sumer and Akkad*, p. 244 ff., had personally inspected the site from which the stele had been brought and was keenly aware of its significance, but final proof was lacking until the find of Dr. Naub.

<sup>2</sup> These texts are not the original, though an early Babylonian copy, circa 2200 B. C., cf. Unger, loc. cit. Thus the spellings of the names may have varied somewhat, permitting the suggestive equation of Unger, loc. cit.: Uraa — Uriaa, but cf. Smith, URI p. 80.

<sup>3</sup> Smith, however, in URI p. 81, doubted the validity of this late text for drawing conclusions for such an early period. It is known, from *KBo* III 13 = *2BaTU* i No. 3, obv. col. I l. 12, that a Ma-da-ki-na was king of



More than 1500 years later, for precisely this same Diarbekr region, the Assyrian king Shalmaneser I reported the Uruatri-lands. Here the word is written out *urā-ri-aṭ-ri* (var. *ū-raṭ-ri*) ; see AOB I p. 113 ff. Later the old sign for Akkad, as stated above, is often used by the Assyrians for this Urartu land. This remarkable consistency in terminology (Akkadian *Armi-num* and Assyrian *Uru-ri-aṭ*) is odd. Stranger still that for this same region, roughly speaking, the Behistūn Inscriptions of Darius I should equate Uraštu (= Urartu) with a form Armina (Arminiya), from which, through Herodotus, the modern term Armenia directly originates.<sup>7</sup> Is this form Armenia another example of an ancient land-name, long out of use, brought to life again? If the correlations above are not accidental and if my suggestion that Armi-num seems to have been in the Pir Hüseyn region is correct (nothing can be adduced either to prove it or disprove it in the present state of our knowledge), then a possible origin of the term Armenia, for which no satisfactory etymology has been offered (cf. Struck, *Encyclopaedia of Islam*, p. 436), may herein be found. Final proof can only be found from archaeological excavation in the Pir Hüseyn tells. It would seem to be one of the most promising sites for an archaeological expedition.

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Armi-num instead of the Rid-Adad mentioned in UR I 275; cf. Hrouzý, "Naram-Sin et ses ennemis d'après un texte Hittite" AOr I (1920), pp. 65-70. But this apparent confusion of names does not, in my opinion, negate the geographical validity of the texts in question. Cf. Goetze, *Kleinasien* in "Kulturgeschichte des Alten Orients," *Handbuch der Altertumswissenschaft* III 1, 3 (1933) p. 91 ff. and the literature there cited.

<sup>7</sup> See *British Museum: The Inscription of Darius the Great at Behistūn* (1907), p. 1 for references. Another inscription of Naram-Sin, published by Thureau-Dangin, *Revue d'Assyriologie* 8 (1911), 199-200, states that he "fought against Haršamatki, lord of Aram and Am." Whether this form Aram may be somehow equated with Armi-num (so, for example, Gelb, loc. cit.) and whether it can be correlated with such a name as the first king of Urartu, reported in the inscriptions of Shalmaneser III (see Luckenhill, *Ancient Records* 1, 560 et passim) or the Aram of Armenian mythology, are suggestive problems with which I am not now prepared to deal.

*Another Occurrence of the Alleged Ancient Name of Sāmarrā*<sup>1</sup>

J. Lewy<sup>2</sup> has recently discussed an important unpublished Sennacherib inscription now in the Walters Art Gallery of Baltimore, which, according to its own testimony, comes from Surmarrāti. This occurrence marks the third appearance, so far as I am aware, of the city Surmarrāti in cuneiform literature, all three dating from the late eighth, or seventh, century B. C. E. Forrer,<sup>3</sup> who was the first to equate ancient Surmarrāti with Sāmarrā, refers to the two occurrences of the name which were then known in the list of districts K 4384 (*JIE* 53, no. 1), obv. (1),<sup>4</sup> col. I, line 11 (following Forrer's numbering of the lines), written *su-ur-mar-ra-ta*; and in *ABL*, Vol. IX, no. 944, line 5, as *su-ur-mar-ra-ta*.<sup>5</sup> The equation of the ancient name with Sāmarrā was based on the relative position of Surmarrāti in the above-mentioned list of districts.

But Surmarrāti is also mentioned in another epistle in the same Harper corpus of letters from the last century of the Assyrian Empire which furnished one of the three previously known writings of the name, but the reading has hitherto remained unnoticed because it has been misread in a number of different ways. Waterman<sup>6</sup> and Pfeiffer<sup>7</sup> both read the name of the city which occurs

<sup>1</sup> Part of a paper presented under the title "Two Cuneiform Place-names" before the Near Eastern Section of the American Oriental Society at its Annual Meeting in New Haven, April 15, 1936; see *JAOI* 56, 404.

<sup>2</sup> At the Ann Arbor meeting of the American Oriental Society, April, 1936 (see *JAOI* 55, 363); "The Chronology of Sennacherib's Accession," in *Miscellanea Orientalia Dedicata Antonio Deimele* (Roma, 1935) = *Analecta Orientalia* 12, 220.

<sup>3</sup> *Die Provinzeinteilung des assyrischen Reiches* (Leipzig, 1920) 104. A sceptical attitude toward Forrer's identification is taken by Welsbach in *Paulys Real-Encyclopädie d. class. Altertumswiss.*, Neue Bearb., 2. Reihe, 7. Halbband (Stuttgart, 1931), 606-6, s. v. Samara.

<sup>4</sup> Following Forrer, op. cit. 62. For a valuable collation of this text, see E. Burrows, *JRAS* 1924, 706.

<sup>5</sup> So Forrer, op. cit. 104; Unger, who cites possible parallels to the name in *RLA* I 413b, reads Surmarrāta (i. e., *sur-mir-ra-ta*). In this connection it should be remembered that we are dealing with the Babylonian spelling of the name which is represented by the Assyrian spelling (in the district list) *su-ur-mar-ra-ta*.

<sup>6</sup> *Royal Correspondence of the Assyrian Empire*, Part I (Ann Arbor, 1930), 374. Cf. now Part IV (Ann Arbor, 1936), 184, where my reading is indicated.

<sup>7</sup> *State Letters of Assyria* (New Haven, 1935), 129, no. 175.

in the Babylonian letter, *ABL*, Vol. V, no. 539, line 8, as *4aš-šá-mar-a-ti*.<sup>8</sup> As Waterman remarks in his commentary<sup>9</sup> to the text in question, "Ashshamarati occurs only here." Toffteen,<sup>10</sup> years before, had read this name as *Ašša*; also Ebeling, in *ELA* I, 170a, thinks that we should read *Ašša* here. Streck,<sup>11</sup> in his criticism of Toffteen's work, proposed the unhappy reading *4Aššur(AŠ) šá*, etc. As may be seen from Harper's edition of the letter in cuneiform type, the two signs *AŠ.ŠÁ* at the beginning of the city-name are written not far apart. When we read *AŠ.ŠÁ* as one sign, the resultant reading is *4šur-mar-a-ti*. The probability of such a reading is enhanced not only by the fact that the name *Aššamarati* never occurs elsewhere, but also by the circumstance that the letter under discussion is written in the Babylonian script and language of the period, which is only to be expected (if Farrer's identification is correct) from the geographic position of *Sāmarrā*, about 130 kilometers north of Baghdad on the eastern bank of the Tigris.

It was pointed out many years ago that the Arabic spelling *sarra-man-raʿā* "Delighted-Is-He-Who-Beholds" for *Sāmarrā* rests upon an Arabic popular etymology. One Arabic tradition, in a valiant effort to square the orthography with the pronunciation, maintains that the city was called *sarra-man-raʿā* when it flourished, and, after it was abandoned, *saʿā-man-raʿā* "Displeased-Is-He-Who-Beholds." Other curious etymologies, including some with a Persian basis, are listed in the second part of M. Streck's *Die alte Landschaft Babylonien nach den arabischen Geographen* (Leiden, 1901), pp. 183-4.

That the excavations at *Sāmarrā* conducted before the War by Sarre and Herzfeld brought to light nothing from the Iron Age, if indeed there was a settlement in that period at the site, is not surprising when it is realized what a relatively short period of time was spent in actual excavation and how immense the place was in the ninth century A. D.

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<sup>8</sup> Quotations of transcriptions based on other usages are here given according to the Thureau-Dangin system of notation.

<sup>9</sup> Op. cit., Part III (Ann Arbor, 1931), 187.

<sup>10</sup> *AJSL* 21. 87.

<sup>11</sup> *AJSL* 22. 200. R. P. Boudou, *Orientalia* 36-38 (Rome, 1929), 27, follows Streck.

*Two Far Eastern Bibliographical Notes*

Professor K. S. Latourette presented at the autumn meeting of the National Academy of Sciences in 1936 a Biographical Memoir of the late Berthold Laufer. The memoir is much fuller than that published in this *JOURNAL* 54 (1934), 349-351 (with bibliography, pp. 352-362), and the bibliography includes a number of items not previously mentioned. The memoir has been published in pamphlet form, and contains twenty-five pages.

At the beginning of the century there was little interest in Far Eastern studies in the United States, and some of the work that was done in this field is not generally known. In 1909, Sadajiro Sugiura presented a doctors' dissertation at the University of Pennsylvania in Philosophy, entitled "*Hindu Logic as Preserved in China and Japan.*" Part I is concerned with the development of Hindu Logic and its introduction into China and Japan. Part II deals with the Logic of Dignāga, and Part III includes a number of critical notes. Sanskrit, Chinese and Japanese sources are considered, and there is a bibliography. In spite of the fact that the romanization is faulty and misleading, the study is valuable, and should be better known. Dr. Sugiura has since had a distinguished career as an educator in Japan.

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## REVIEWS OF BOOKS

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*The Buddhacarita: or, Acts of the Buddha.* Part I, Sanskrit Text. Part II, Cantos i to xiv translated from the original Sanskrit supplemented by the Tibetan version, together with an introduction and notes. By E. H. JOHNSTON, D. Litt. (Panjab University Oriental Publications, Nos. 31, 32.) Calcutta: BAPTIST MISSION PRESS, 1935-6. xi + 165 and xviii + 232 pp.

As a work of literary art, Aśvaghoṣa's poetic "Life of the Buddha" is considered one of the glories of Sanskrit literature. Its early date (between 50 a. c. and 100 a. d.) and its Buddhistic character give it importance also for linguistics and for the history of Buddhist and general Indian thought.

There was great need of a new edition and translation. The only previous edition of any value was that of Cowell (1893); Cowell's own translation, and the several others that have appeared, were mainly based on this, which even in its day was not a first-class performance. Cowell's manuscripts obviously presented a very faulty text; he made some emendations, but not enough, and not always the right ones. Many more have been proposed by others, and not a few of these are now proved right. Cowell had no personal knowledge of the Tibetan or Chinese translations, which (especially the former) are of prime importance for establishing and interpreting the Sanskrit. He used them little, and only at second-hand. F. Weller has now edited and translated the Tibetan; and Else Wohlgenuth has published a scholarly rendering of the first two chapters of the Chinese (for which Beal's rendering in SBE xix is of little value).

Mr. Johnston shows that he has very good first-hand knowledge of both the Tibetan and the Chinese. While he naturally uses Weller and Wohlgenuth, he handles them with complete independence and scholarly competence; his use of most of the Chinese was necessarily direct and virtually unaided by previous publications. These advantages alone, with the numerous contributions of various scholars since Cowell's day (there is a large bibliography of notes on the text), would have enabled Johnston vastly to improve on Cowell. But perhaps his greatest advantage is the possession of a

rotograph copy of a much older and better manuscript than any known to Cowell. This ms. A, discovered about 1909, is in fact a direct ancestor of Cowell's modern copies, so that, as Johnston proves, those copies, on which Cowell based his edition, can and should now be ignored, except for a few places where A became illegible, or (in one instance) lost a couple of leaves, after the copy was made from which Cowell's codices were recopied. Incidentally, A lacks the first page with verses 1-7 of Chapter i, and so disposes of any remaining doubt that the opening part of i in Cowell is a modern addition, presumably by the paṇḍit Amṛtānanda, who (as he tells us himself in the colophon) also undertook to complete the work, which in A is lost after xiv. 31 (32 in Cowell), exactly the point where Cowell's text ceases to agree with the Tibetan. We have therefore in Johnston all of the Buddhacarita that now exists for us in Sanskrit, namely i-xiii (except i. 1-7) and xiv. 1-31. (In the translation, xiv is completed on the basis of Tib. and Chin., bringing the story to the attainment of enlightenment.) Amṛtānanda's beginning of i and completion of xiv, and his xv-xvii, which Cowell included, are properly thrown out. The original work had 32 chapters, as Tib. and Chin. show. One block of eleven verses (ix. 42-51, cf. note on 47) and several stray verses elsewhere were lost in the archetype of Cowell's mss., but are present in A and hence in Johnston.

Johnston's work comes very close to being definitive, unless and until important new materials are discovered. Of course many doubtful points remain, as he fully recognizes. Discordances between A, Tib., and Chin., particularly the first two, often leave one uncertain as to the true original; and even when all seem to agree there are many puzzles. But all that a thoroly competent, intelligent, and industrious editor could be expected to do has generally been done. Fairly often, different ideas will occur to the attentive reader; but usually it will be found that Mr. Johnston has anticipated and rejected them. One may occasionally differ with his decisions, but very seldom can one accuse him of overlooking any reasonable possibility.

The long Introduction to Volume II discusses capably and interestingly all relevant matters about Aśvaghoṣa and his work. Linguistically inclined students will be particularly interested in pp. lxvii ff., on the poet's language. It is clear that it is in the main standard, even Pāṇinian, Sanskrit. Special relations to Buddhist

Hybrid Sanskrit occur chiefly in the matter of vocabulary, and even there rarely. An interesting syntactic instance is the isolated use of the *yaṁ . . . tena* construction ("where . . . there," cf. BSOS 8.501) in vi. 55. (Dr. Sukthānkar informs me that instances of this occasionally occur in the Sanskrit Epic.) A lexical matter not mentioned by Johnston is *abhūta* "false" (of statements, Skt. *asatya* or *amṛta*) vi. 36; this, and its opposite *bhūta* "true," are definitely Buddhistic. In i. 74 J. might have quoted Pali *tāḷa* "key" in support of his correct interpretation of *tāḷa*.

The translation never sacrifices accuracy for style, yet it seems to me hardly to deserve the author's modest epithet "pedestrian." It is not poetic, nor should it have been. But it shows real feeling for the English language.

In my opinion it would not be profitable to list all the passages where I should incline to a different construction or interpretation of the text, since most of them concern admittedly doubtful matters, and I could rarely add considerations of which Mr. Johnston was unaware. The following selection contains in large part minor oversights, with a few matters on which I hope to have made real contributions.

Text: — In iii. 58, v. 39, viii. 11 and 42, *nīdāmya* of A is emended to *nīdāmya* (which is Cowell's reading in the last passage); contrariwise, in viii. 8 and 14 *nīdāmya* of A is emended to *nīdāmya*. I find no statement in J. to explain or justify this, but I infer from his translation and notes (see also his note on Translation, xi. 20) that he assigns the meaning "hearing" to *nīdāmya* and "seeing" to *nīdāmya*. I know of no ground for this or any distinction between the two forms, or for ever emending one to the other. A theory which requires so many emendations in one relatively short work should at least be bolstered by other evidence. In iv. 12, A *cōturyārāpasasapadā*, Cowell's mss. *cōturyārā rā*; J. emends metri gratia to *rāpasōturyasapā*, which seems to me too violent a change to be justified by so slight a metrical consideration (there are parallels for the "flaw," see Hopkins, *Great Epic*, 238 and 457). The Tib. apparently had *cōturya-* {*māras*} before *rāpa*. In iv. 89 and v. 12 A's *dharmo*, *dharmā* need not be emended to *dharmā*, since the stem *dharmā* is often substituted for *dharmā*. On vi. 32: the repetition in the following verse of *deśa*, but referring to Yāśodharā and not to Gautamī, seems to favor A's *deśa* rather than Tib.'s *deśa*. In vi. 48 *parṇaparavā* may perhaps be kept: "and since (the people of) this world go(es) on disappointing (= abandoning, separating from) one another. . . ." In vii. 36, did J. intend to read *mārgyāśa* with Cowell (and A?), instead of *yāśamāśa* with Tib.? This seems indicated by his own translation, as well as by his rendering of Tib. in note to text. In viii. 1, could not A *cikāśa* be kept, meaning "was

abandoned, given up" (BR. s.v. *kṣip*, 3) ? J.'s *oikēiye* assumes a form (perfect of *kṣi*, *kṣiyate*) not recorded in literature.

Misprints, or what I take to be such, not corrected in the Corrigenda (end of Vol. II): III. 43, read *jētaḥ*; IV. 18, *deivatair*; IV. 74, *śaśāṭapāḥ*; V. 55, *\*ākulaśakṛpāyuktakṛpā*; V. 57, *apard* no *babbar*; VIII. 10, *yudhāpānte* (?); VIII. 83, add *hi* after *bāhavo*; XI. 31, add *kāmeṣu* after *teṣu*.

Translation: i. 83, a double entente in *paramabhāṣaṃ* is ignored (cryptically, "for the last existence or birth"). In III. 15, *prāśūda-* is omitted. In IV. 10, it seems to me that S. vii. 30 proves (with its *kāṭiṣa redpavedhāḥ*) that *kāṭisandhā* is not a proper name. On IV. 24 with note, cf. Bloomfield, JAOS 39. 290 ff. In IV. 85 *jagat*, and in V. 14. *jagato*, seem to be omitted. In note on V. 12 read "Pali . . . *vijigucchati*" (and I am obliged to consider the interpretation rather forced). In V. 52 *śālabhāṣṭikā* or *śāla* (no ms.) means simply "statue," rather than "statue of a *śāla*-plucker"; it is correctly rendered by Vogel, AO 7. 208, to which J. refers in his note. See Vogel's article on the whole subject. In VIII. 21, *dīvi* is omitted. In VIII. 85 (cf. note) it seems to me that *eva* means, as so often, "absolutely, in spite of all to the contrary." Here what is implied is, "in spite of the considerations we have urged." Precisely as *athāva eva* ix. 80, where J. omits *eva* in translating; it implies "in spite of all the arguments advanced"; cf. also xi. 11. In ix. 43, *akṛdyair* . . . *kṛpāśarpakā* "with invisible ( lurking) cobras," rather than "with loathsome black snakes." In xi. 31, *śaśāṭapadāḥ* is omitted. In xii. 3, *śaśaśaśaśa* is omitted. In xii. 85-86, the interpretation of *śaśāṭapadāḥ* and of *śaśāṭapāṇi* *śaśaśaśaśa*, while essentially correct, would have been clearer by a reference to the familiar Pali *śaśāṭapadāḥ* (-*āṭapāṇi*), which is precisely what is meant, as is proved by the fact that this stage "follows after *śāṭapāṇi* (Pali *śāṭapāṇi-āṭapāṇi*)," as in DN. xvi. 6. 8 (ed. II p. 156). The (Buddhist) Sanskrit *śaśāṭapadāḥ* (-*āṭapāṇi*) is used in prose, LV 243. 17, 244. 13, 245. 3, 403. 10.—In xiii. 20, note, I do not understand why *kāṭandhāṣṭa* "should mean not 'carrying kāṭandhas in their hands' . . . but 'having hands like kāṭandhas.'" This seems to impugn a common use of *haste* in compounds which is illustrated in the very next verse. In xiii. 30 and 37, *-akṛta-* is surely not "torn" but "distorted" (with anger); Tib. 30 renders by *ram-par yag-pai*, "very crooked." In xiv. 29, Tib. renders *śaśāṭapadāḥ* by *śin-tu non-pu*, "completely conquered," i.e. overwhelmed (by desire), and I cannot doubt that this is what the Skt. word meant. It is not hard to derive it from well-established meanings: "completely gone over, swept over (as by a hostile army)," the I confess I have found no close parallel. J.'s rendering, "reaching the limit of longing," seems to me to ignore the instrumental case of *śāṭa*.

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*The Cultural Heritage of India.* A symposium by some 100 authors. 3 vols. Belur Math, Calcutta: SRI RAMAKRISHNA CENTENARY COMMITTEE: n. d. [1937]. Pp. xxvi + 1917, with 164 illustrations.

*The Legacy of India.* Edited by G. T. GARRATT, with an introduction by the Marquis of Zetland. Oxford: CLARENDON PRESS, 1937. Pp. xviii + 428, with 24 plates and a map.

These two publications, covering practically the same ground, present a remarkable and interesting contrast. The "Cultural Heritage" consists of a hundred articles, all without exception contributed by Indians, of whom nine are Swamis connected with the Mission, and the remainder almost all of university and professorial rank. The unanimity of the several authors' points of view produces a remarkable effect of unity in the book itself; the various aspects of Indian culture, philosophic, religious, scientific, social, historical, and artistic are considered by writers who are authorities in their special fields, not as ends in themselves but from the point of view of their function as means to "the realisation of the supreme goal of life."<sup>1</sup> It is probably only in India at the present day that the supremacy of spiritual values could have been so consistently asserted throughout a work to which so many authors have contributed. It is on the one hand a tribute to the enduring power of Sri Ramakrishna himself, and on the other proof of that essential unity of India that has persisted through millennia unaffected by political vicissitudes, and comparable to that kind of cultural unity that came near to being realised in Europe before the rise of the separate nations. Many of the articles are admirable introductions to and critical expositions of the subjects discussed in them. Modern political problems and the presence of Europeans in India are virtually ignored. What is here described without any trace of chauvinism is an India of the Indians, that India that has "bowed low before the West in patient, deep disdain," and can afford to wait. There will never come a time when there will be no *sādhus* in India. In the present context it is perfectly appropriate that one of the longest, and in some respects the most important of the contributions should have been Swami Nirvedananda's discussion of "Sri Ramakrishna and spiritual renaissance," in the course of which the life is related at some length, and with a very attractive directness of approach, despite the irrelevant quotations

from H. G. Wells. It is worth while to observe how profoundly Ramakrishna's conception of social service differed from that sort of altruism and philanthropy that has nowadays so largely taken the place of religion in the West. For Ramakrishna, to love one's neighbor "as oneself" had a far other meaning than for us; a meaning not so much moral as metaphysical, and based on the almost universally accepted Indian belief that the true self of oneself and of all other beings, whether animate or inanimate, is God alone; Ramakrishna understood quite literally what we regard as a figure of speech, that "Inasmuch as ye have done it unto these, ye have done it unto Me." Ramakrishna believed, what most of us who believe at all are afraid to believe, that whoever is joined unto the Lord "is one spirit"; and all those of all sorts who knew him personally were convinced that he had again and again had a direct experience of this in his own person.

The "Legacy" consists of fourteen articles, nine of them by Europeans. It is not so much the legacy of another culture to Europe that is here discussed (as was the case in other volumes of this useful series), but the heritage of India in the sense in which this word is employed in the title of the volume reviewed above. The volume falls behind the "Cultural Heritage" in total achievement, but separate articles, in particular Strangways' "Music" and Clark's "Science" are of outstanding value. In Professor Das Gupta's "Philosophy" it seems to us that the emphasis on Buddhist heterodoxy and the statement that the Buddha "did not accept . . . the existence of an eternal and immortal soul" are rather out of date, if we assume that he is rendering *ātman* by "soul"; we are much rather in agreement with Mrs. Rhys Davids' view (JRAS. 1937, p. 259) that the Buddha took the existence of the *ātman* for granted, and concerned himself mainly with the destructive analysis of whatever-is-not-the-*ātman* (*anatta*). Canon Galpin misunderstands the history of India when he speaks of a "popular revulsion from Hinduism" as taking place in the fourth century B.C. Mr. Garratt's "Indo-British Civilisation" includes an interesting discussion of Anglo-Indian literature in the course of which he remarks that "some interpreter was needed for that curious phenomenon, British rule in India. Kipling supplied the necessary exposition in a manner most flattering to English pride, and in forms . . . which were easily assimilated," adding that most of his followers have had "the same tendency to make their real characters Euro-

peans, while Indians form a shadowy background, types rather than human beings." I am glad that he is able to say that "only very banal versions" of Tagore's poems have yet appeared in English. The Marquis of Zetland, quoting Mr. Garratt, remarks of the period of British rule that "it comes with something of a shock to discover from the concluding chapter of this volume how little has been added during that time to the legacy of India in the sense in which that word is here employed." In this sense the second volume may be said to confirm the impressions created by the first.

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*Gaṇeśa*. By ALICE GETTY. With an introduction by A. Foucher. Oxford: CLARENDON PRESS, 1936. Pp. xxvii + 103, with 42 pl. (2 col.) and 8 text figures, glossary, bibliography, and index.

If not absolutely exhaustive, this excellent volume assembles almost all that is known of the rather mysterious elephant-headed Indian deity whose image does not appear in art before the Gupta period. We are in agreement with those who hold that the iconographic form is a later realisation of the *gaṇāpāṇi gaṇapati* — Brahmanapati of RV. II. 23. 1 and AB. I. 21; the Dandin *nakratuṇḍa* of TA. X. 1. 5; the collective Vināyaka, son of Ambikā, in the Yājñavalkya *śmṛti*; and Gaṇeśa or Gaṇeśvara as a designation of Śiva in Mbh. To these references may be added the attribution of an elephantine quality to the Maruts (*hastibhū* in RV. I. 64. 7) and Indra (*mṛga va hasti*, RV. IV. 16. 14), that of an "elephantine glory" (*hasti-varcas*) to the Sun in AV. III. 22. 1, and the prayer for an "elephant's force" (*hastini . . . tvīṣiḥ*) in AV. VI. 38. 2, a hymn for "glory" (*varcas*) addressed to Bṛhaspati and Tvīṣi jointly. These references taken together afford an adequate "prescription" for many, if not all, of the characteristics of the later Gaṇapati. All this would be in agreement with the general rule that what in RV. are essential names of the supreme deity become later on either the designations of classes (as in the case of *yakṣa* and *gandhārva*) or the personal names of individual deities (as in the case of *kāma*) and if in the later Gaṇapati there is an admixture of popular elements, all this means that "Gaṇapati" was rather reduced in status than later on "raised to the rank of Creator."

As iconographic precedents we have at Bodhgayā representations of ithyphallic Yakṣas with elephants' ears and at Amarāvati a *gaṇa*

or *yakṣa* with an elephant's head. As to this last, Miss Getty (p. 25) is wrong in saying that the representation "has neither trunk nor tusk"; the head is shown in daring full-face perspective, with the trunk raised (the nasal opening can be clearly seen), and tusks represented by circles just above the mouth. Ganeśa's pot-belly is also a definitely *Yakṣa* characteristic, a mark at the same time of procreative and of ingestive power, and certainly not from an Indian point of view a defect, cf. RV. I. 112. 17 where the *ṛṣi* *Patharvā* is "mighty-bellied" (*jātharasya majmanā*, not inaptly rendered by Griffiths, "in his majesty of form").

Miss Getty is also mistaken (p. 33) in casting doubt on the identification of Śrī-Lakṣmī in the "Trinity of Fortune"; the elephants with the vases can be distinguished in the Philadelphia example, and can be seen more clearly in Mr. Heeramanek's example; see my "Trinity of Fortune" in (the Pennsylvania) University Museum Bulletin, II. 1930, where both are reproduced.

To the Bibliography should be added the article just mentioned, and: Scherman, L., "Dickbauchtypen in der indisch-ostasiatischen Götterwelt," *Jahrb. as. Kunst*, I. 1924; Salmony, A., "Two representations of the Hindu pantheon and their origin," *Journ. Ind. Soc. Or. Art*, I, 85-88, 1933; Chapin, H., "A Chinese Ganeśa," *ibid.* III. 51-52, 1935; and Washburn, G., *Master Bronzes*, Buffalo, 1937 (pls. 34, 104).

All of these are very minor criticisms. The book is admirably planned and well produced and will be indispensable as a work of reference for every student of Indian iconography and religion.

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*Études de Morphologie iranienne I. Les Composés de l'Avesta.*

Par JACQUES DUCHESNE-GEILLEMIN, Aspirant du Fonds national belge de la Recherche scientifique. (Bibliothèque de la Faculté de Philosophie et Lettres de l'Université de Liège, Fascicule LXXIV.) Liège: FACULTÉ DE PHILOSOPHIE ET LETTRES; et Paris: LIBRAIRIE E. DAOZ, 1936. xi + 279 pages.

Three outstanding works of Chr. Bartholomae have acted to stabilize Avestan studies for at least three decades: his *Vorgeschichte der iranischen Sprachen* (writing completed in 1894) and *Awesta-*

*sprache und Altpersisch* (writing completed in 1895), forming the first two parts of the first volume of Geiger and Kuhn's *Grundriss der iranischen Philologie*, published in 1901, and his *Altiranisches Wörterbuch*, which appeared in 1904. Advances from their standpoint have been but slight, until quite recently, when É. Benveniste of Paris began to overhaul the entire field of Avestan morphology: witness his article on *Les absolutifs avestiques* in *Mém. Soc. Ling.* 23. 393-402 (1935) and his volume *Les infinitifs avestiques* (Paris: Adrien-Maisonneuve, 1935), in both of which his keen critique leads to new and better results.

Under Benveniste's influence, M. Duchesne-Guillemin has made a thorough examination of the compounds found in the Avesta; over 2700 such words are listed in his index (pages 327-367). He has studied them by the classifications of the Hindu (Sanskrit) grammarians, following mainly the order found in Wackernagel's *Altindische Grammatik*. The Indic arrangement fits also the Avestan compounds; but, naturally, this commonplace conclusion, while the treatment greatly supplements the treatment by Bartholomae in *Vorges.* 148-151, is accompanied by numerous improvements in detail.

Thus he finds (pages 6-16) that the prior term of a compound is never in the nominative singular: those in *-ō* have long been recognized as false redivisions of the words, those in *-ā* are miswritings for *-a-*, those in *-ē* are the product of an old sandhi or are grammatically justifiable negatives plural, or are etiological formations. He proves (pages 24-25) that the supposed negative prefix *an-* is to be otherwise analyzed. Here and elsewhere he corrects many corrupt readings, restoring to the words their proper forms. The only Avestan innovation which he finds in nominal composition, is (page 203) a rare type in which the prior term is a perfect participle; but these are always from perfects with present meaning, so that there is no new semantic combination.

The third part (pages 211-226) deals with the function of the compounds in the Avesta, and is most instructive. Dvandvas are rare except in the Gāthās, where they denote pairs of divinities or of abstracts, or of naturally associated objects. Except for proper names (which are compounds of the original Indo-European types), the Gāthās use the compounds chiefly to express moral ideas, among which complementary pairs are frequent (e. g., *daśmanah-*, *humanaḥ-*); the later Avesta uses mostly traditional descriptive

epithets, which serve to fill out the metrical units of eight syllables. The Gāthās, he says (page 231), are visibly seeking, in making the new compounds, to sum up in an elliptic form the new ideas for which they were seeking to win acceptance.

The author promises two further volumes, one on the formation of nouns in the Avesta, and the other on their inflection. We can do nothing better than express the wish that they may be up to the standard of his present work.

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*Hebrew Origins.* The Haskell Lectures for 1933-34: The Graduate School of Theology, Oberlin College. By THEOPHILE JAMES MEER. New York and London, 1936: HARPER & BROTHERS. Pp. xi + 230.

This is a volume based upon public lectures at Oberlin from the hand of a thoroughly competent scholar, and it finely meets the obligations of such double auspices. The book can be read with increasing interest by that class of citizens who are called "intelligent laymen," and equally by those who are dubbed "professors." There is a tendency, incited no doubt by the business end of publishing houses, to depress the scholarly in favor of the "popular appeal." But in this case Professor Meer and the Messrs. Harper have put forth a volume that does not hide its light under a bushel, nor on the other hand "scandalize" the reader with its learning. In its series of chapters covering wide fields the author presents his facts with full documentation all up to date, and indulges in extensive discussion, where warranted, of controverted points, with objective presentation of the various theories and results along with well argued solutions of his own. He is not possessed by some one simple equation which solves all the problems.

As its title tells, it is a Book of Origins, and so are the six chapters: the Origin of the Hebrew People, of their Law, of their God, of the Priesthood, Prophecy, and finally, of Hebrew Monotheism. In ch. 1 the author makes a wise confession for all who would treat the early history of Israel (p. 2): "All of biblical tradition cannot be accepted as a fact, but facts do lie behind and within the biblical records, and it is the task of the historian to discover these facts

and give them their true setting. And that is no easy task, and larger knowledge does not always help that task, but complicates it and makes it more difficult, because the more we know, the less we know, or to put it in another way, the more we know, the more we know there is to know. . . . And so it is with Hebrew origins." All of which is true philosophy.

The same chapter gives an excellent picture of the ethnic and political turmoil of Canaan in the second millennium B. C., and most usefully sums up the assured facts of archaeology, e. g. the lucid presentation of the chronology, pp. 39 ff. Ch. 2, dealing with Hebrew law (taking its place here because of the antiquity of the Babylonian law codes), while giving full credit to the influences via the Canaanites from Babylonia, recognizes the individual characteristic of the Hebrew law, partly as arising from Bedouin tradition, partly as from the original element of priests and prophets who made Israel's law in contrast with the purely political and economic motives of the Mesopotamian codes; "the Hebrew motive was to better the social and religious life of the people." The author does not explain how these loftier-minded priests and prophets arose, except for their traditional connection with certain shrines. Forerunners of the Prophets of the Book were such men as Elijah and Nathan and Samuel. In a later chapter (ch. 4) the priests are not presented in a very favorable light; they are opportunistic ecclesiastics, while the prophets arise definitely first as political antagonists to the Philistine tyranny (p. 153). But he admits (I. c.) that Moses might well be regarded as a prophet, and Deborah quite certainly so. The query naturally arises whether this religious and ethical motivation was not early and indeed aboriginal in Israel.

Chapters 4 and 6 discuss the Hebrew God and Monotheism, i. e. the essential religion of Israel. Dr. Meek makes the most of the theory of the locally and chronologically distinct invasions of Canaan by the people that came to call itself Israel, the one connected with Joshua and passing from Transjordan into northern Canaan, the other from the south (cf. Jud. 1) and establishing the tribe, ultimately the kingdom of Judah, originally distinct from North Israel. The North, it is alleged, was practically pagan, as its tribal names testify, Rachel, Gad, Asher, Zebulun, Issachar (p. 104). Only "when Judah came to dominate all the Hebrew tribes, as it did in the time of David, then and then only did Yahweh become the god of all the people. To the Northerners, then, Yahwism was



a southern cult . . . rather recently come to Israel" (p. 114). Somewhat belatedly Dr. Meek denies the contention of some that Yahweh was not known at all in Israel until the time of Deborah, but "the practical absence of Yahweh names in the North down to the time of David . . . would indicate that the Yahweh cult was not largely known until the Judean conquest of Israel by David" (p. 141). However the Song of Deborah is an unescapable monument to the pristine Yahwistic character of the Northern religion, while oddly enough it avoids all mention of Judah. And the episodes of Gideon, Jephthah, Samson, Samuel, and Sami all belong to the North. Similarly there is a dilemma with the tribe of Levi (cf. pp. 118 ff.). The tribe should have belonged to the serpent-cult (Levi = Leviathan), and the Levite Moses set up a serpent standard in the desert (Num. 31: 5 ff.); Aaron set up the golden calf (Ex. 32), just like Jeroboam at Bethel; a certain Jonathan, grandson of Moses, was priest of the idol at Dan (Jud. 18); and yet the tribe became the sacerdotal caste in the non-idolatrous Judean temple. They must have been clever ecclesiastics, equally uninterpretable in the records and to the critics. But do family characteristics and social movements always proceed in straight lines?

On p. 87 the author denies the Kenite hypothesis for the origin of the God and the distinctive character of the religion. That is well. But on the positive side the reviewer thinks that the author might have made more of Moses, as "a man of God," through whose inspiration Israel was led into its unique religious destiny. For monotheism has never been attained through eclecticism, as the history of the Mediterranean world shows, but through the innovation of a spiritual genius, who, like Elijah, affirms and equally denies. On pp. 190 ff. Dr. Meek presents his view of the origin of Hebrew monotheism. He denies that Ikhnoton's reformation had any influence upon Moses, and proceeds to state that "it is not at all clear that Moses himself was a monotheist. . . . The most that we can claim for Moses is henotheism" (p. 192). The last term he repeatedly uses, but once he exchanges for it another that is far more exact, on the last page, namely "monolatry." Indeed the creed of the First Commandment with its injunction of exclusive devotion to one God, whose name is Jealous, is the key of the whole future development with its affirmation and its equally protestant denial, the first durable case in the history of religion, its implications left to be hammered out on the forge of Israel's later history.



The reviewer thanks the writer for a book that not only gives facts and theories but makes him think—which is the chief pleasure in reading.

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*Inscriptions from Alishar and Vicinity* (The University of Chicago Oriental Institute Publications Vol. XXII — *Researches in Anatolia* Vol. VI). By IGNACE J. GELB. Chicago, Ill.: THE UNIVERSITY OF CHICAGO PRESS, 1935. Pp. xv + 84, LXIII plates. \$6.00.

This volume of the "Researches in Anatolia" contains the epigraphic material found at Alishar during the years 1927-1932 by the expedition of the Oriental Institute of the University of Chicago. As had been indicated in the previous communications of the excavators, almost all inscriptions belong to the class of the so-called Cappadocian tablets which reveal the language, the law, and the economic development of Assyrian merchants settling and trading in Eastern Anatolia in the 20th century B. C. Since these short business letters and juridical and administrative documents always include some information about the contemporary non-Assyrian population of early Cappadocia, the prompt publication of the new material will be welcomed by many. Historians and linguists will take a particular interest in nos. 5, 12, and 49. The greater part of the letter no. 5 concerns the release of certain *habirū*, i. e. mercenaries, in the service of the local ruler of Šalaḫšawa who apparently had penetrated into the territory of another prince. No. 12, a legal decision of the *kārum* *Kaniš* appears to reflect the principle of the Assyrians to redeem those of their compatriots who had become bond-servants of the *na'dū* or "evil-doers," as the Assyrians seem to have called the "barbarian" natives of Cappadocia. The unique document no. 49 which was written at the time of "Anitta the great prince"—evidently Anitta of Kaššara, the son of Bithanu whose wars are recorded in the Boğazköy inscriptions VAT 7479 and Bo. 9058<sup>1</sup>—deals with six non-Assyrian men and women who had been detained in the house of a police chief

<sup>1</sup> Cf. Hrozný, *Archiv Orientalní* I (1923) pp. 273 ff.; Lewy, *Revue Hittite et Asiatique*, fasc. 17 (1934) pp. 1 ff.

(*herallum rabûum*) at Amkuwa, i. e. possibly the modern Alishar.\*

Compared with the total number of inscriptions found at Alishar, the number of official letters exchanged between the various Assyrian authorities is considerable. We refer, apart from the much damaged tablet no. 17, to the fragment no. 40. Mainly with the help of *TC* 35 (see *MVAeG* 33 p. 245 note a and *MVAeG* 35, 3 p. 183) and a few other texts from Kültepe containing the typical *imburaiati* "he approached us," its opening lines might be restored as follows: <sup>1</sup>[*um-ma ka-ru-um Ka-ni-is-ma*] <sup>2</sup>*a-na ki-r[i-im]* <sup>3</sup>[*Qá-a*]r-qá-ar <sup>4</sup>*û wa-ba-r[a-tim]* <sup>5</sup>*šaher rabi qí-bi-ma [a-na-kam]* <sup>6</sup>*I-áí-Ku-bu-um mâr [C-sur-án-A-áir]* <sup>7</sup>*im-hu-ur-ni-a-ti* <sup>8</sup>*um-ma šu-ut-ma* . . . . .

Fully as valuable as the 53 Alishar tablets are the nine well preserved tablets from Kültepe (nos. 54-62) whose inclusion in the volume justifies the title of the book. The first section of no. 55 duplicates, as the editor has noticed, the Yale tablet *BIN* IV 162; its second section is a copy of the recently published Louvre text *TC* III 159. Lines 5-37\* of the legal text no. 57 recur in the Louvre tablet *TC* III 270 (ll. 14 ff.) which, at the same time, supplements a text of the Blanckertz collection (*MVAeG* 33 no. 253; cf. now *MVAeG* 35, 3 p. 186). No. 59 includes (in ll. 22-30) a very welcome copy of the essential part of a damaged promissory note in the British Museum (*CCT* 11<sup>b</sup> — *MVAeG* 33 no. 24). A considerable part of no. 60 can be restored on the basis of three tablets of the Berlin, Tübingen, and Yale collections respectively (*MVAeG* 35, 3 nos. 323 ff.) which deal with the same law-suit. No. 68 mentions a small amount of copper as "rent for the Tegarama ship" (*ig-ri i-ki-pi-im ša Tê-ga(!)-ra-m[a](!)*). Thus the text indicates that at least a part of the trip from Cappadocia to Assur was made by ship and that Tegarama (which recurs in *KTS* 51<sup>b</sup>, 4 and 6; *CCT* III 44<sup>b</sup>, 8; *TC* III 60, 7 and other Kültepe tablets as well as in the Boğazköy texts) was the place where the caravans embarked—a most natural explanation of the commercial importance of Tegarama recorded in the Bible.<sup>6</sup> A

\* For a detailed analysis of the nos. 5 and 40 see now our forthcoming paper *Old Assyrian Documents from Asia Minor* (II) in *Archives de l'Histoire du Droit Oriental* II.

<sup>6</sup> Since shipping on the Tigris with keleks starts at Diyârbekr and since the name Tegarama possibly means "The Tigris (city)"—the ending *-ma* seems to form adjectives of appurtenance—, it is perhaps not too daring to locate Tegarama near Diyârbekr or to identify it with Diyârbekr itself.

reference in the same valuable tablet to five garments for *Puzur-A-šur mer ru-ba-in* "Puzur-Aššur, the son of the prince" is, as the editor has seen, an important corroboration of the dating of the Kültepe tablets in the time of Šarrum-kên of Assur, as I have maintained since 1926.

Most of the Alishar tablets are poorly preserved and they were not accessible to the editor for a sufficiently long time. In view of this Dr. Gelb's autographs (plates I to XLIX), although they are not always reliable, deserve full appreciation. The general introduction (pp. 1-18), the transliterations, translations, and notes on the texts (pp. 19-69) are much less satisfactory. While his own contributions to the elucidation of this difficult material are very modest—he did not even observe the close relations of nos. 57, 59 and 60 to the above-mentioned duplicates or quasi-duplicates—he criticizes other scholars for mistakes which they did not commit. So he begins on p. 35 a controversy against Lewy and David for having "failed to identify" the root of *wabartum* "station" with Arabic *wbr*, although both authors, in the passages quoted by Gelb, had referred to a review by Christian where the same etymology is proposed. \* Dr. Gelb's translation of *rābi simmiltim* by "chief of the citadel" (p. 52) is accompanied by the remark "Landsberger . . . and Lewy in *RHA* III 1 translate *simmiltum* simply as "Treppe and escalier respectively." The latter had written, however, "Pour *simmiltim* 'escalier' voir Landsberger, *ZA* 41 p. 230-231. *Rabi simmiltim* grand de l'escalier désigne probablement l'officier qui, en qualité de commandant de place, surveillait l'accès à l'acropole et au palais royal . . .," while David, in an article quoted elsewhere (pp. 12; 61) by Gelb himself, had said ". . . sowie den *rabi simmiltim* 'Burgkommandanten' ". It is only in line with these and other misquotations or missing quotations (e. g., pp. 21 f., 29; 34; 43; 54) when a review by Ebelolf is attributed to Landsberger (p. 40) or when the identification of the Old Assyrian term *epāšiltum* with Hebrew *epēšēd*, although resulting from the reviewer's (for the most part unpublished) translations of 750 Kültepe texts for the Chicago Assyrian Dictionary, is mentioned at first (p. 11) without any reference, and later (p. 69) as "a private communication."

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*Té-gu-ru-me-i-um* or a similar Akkadian *niše* may perhaps be read in the damaged text no. 21 (ll. 1 and 6) where the editor tentatively reads *Té-gu-ru-šu-é*.

The translation and interpretation of Old Assyrian tablets as well as the restoration of damaged passages is greatly facilitated by the well-known uniform character of almost all of these texts. Whether they were written in Assur or in Asia Minor, the rather standardized phraseology of the business letters and the formulae of the legal documents remain the same. The first task of every translator in this field is, therefore, to become acquainted with the many hundreds of texts of this class which, since 1919, have been published and discussed by Conteneau, Smith, Thureau-Dangin, Clay, Stephens, Driver, and others. Unfortunately, Dr. Gelb made no serious effort in this direction, as proved by the too frequent occurrence of such elementary mistakes as the following: The current expression *gālka iṣbat* "May your hand seize" — "take over!" is rendered as a new personal name *Gadgali* + *iṣbat*; the word *niṣpaltum* "answer" is confused with *niṣpiltum* "additional payment" (no. 3). The obverse of the damaged case-tablet no. 19 B is taken for its reverse, although dozens of promissory notes of exactly the same type have been transliterated and discussed in older publications. No. 20, another promissory note, contains, in slightly damaged form, the well-known formula *šumma lā iṣḡul kēma awāt hārim šiptam usṣap*; Dr. Gelb replaces [*kēma awāt*] *kā-r[i-iṣ]* by [*a-na*] *Gu-r[i-a]*, i.e. by a personal name not occurring in this document. In no. 40 he takes the expression *per meriamum saḡer rabi* "small great" — "all (of them)" for a new personal name *I-gal* and assigns to the term *ṣu-ḫu-ur-mi-a-ti* a meaning which it never has. No. 6 contains the passage *E<sup>a</sup> -lā-ti iṣ-bi i-dī a-am-ti á gi-iš-tám šé-bi-lam* "E(?) -lafi is grown up. Send me the wage for the maid and a present."<sup>1</sup> Gelb translates "My *allātum* had grown. Itia, my servant girl, and a gift send to me," although this is precluded by the well-known rules by which the use of the verbs *rabāṭum* and *šebūlum* is governed.<sup>2</sup> The fragment no. 45, clearly the beginning

<sup>1</sup> Gelb's autograph gives *a-lā-mi*. A photograph of the original, for which we are greatly obliged to the late Professor Chiera, shows, however, that the sign preceding *lā* is damaged. At any rate, a personal name is required.

<sup>2</sup> This means that the services of the maid who took care of a child are no longer required. Hence she should be paid and given a present.

<sup>3</sup> *Rabāṭum* "to grow up" means only with personal subjects; cf. e.g., the passages *OGT* III 20, 38 ff. and *BIN* IV 9, 20 ff. (*NYAS* 33 p. 7 note b). In the comparatively rare cases in which *šebūlum* "to cause to bring" = "to send" is connected with a personal object, that object refers always to the person who is to be dispatched. — For *allātum* "terebinth" see above.

of a juridical document of a most common type, is taken by him for a letter, despite the fact that letters begin either with *ana A qibima ununa B* or with *ununa A ana B qibima*. The verb *waddu'um* "to make known," "to identify" is erroneously translated by "to inform," the verb *namagurum isti* "to come to an agreement with" by "to find favor" and the verb *itaprusum* (IV 2 from *parāsum* "to decide") by "to agree" (no. 49). In no. 3 B the current sign for  $2/3$  is transliterated as  $3/4$ , although the Old Assyrian scribes used no sign denoting this fraction; later on (nos. 21 and 31) the same erroneous value is attributed, however, to the signs for  $6/5$  and  $1/3$  respectively. *Ilippum*, easily recognizable as an Assyrian spelling of Babylonian *šippum* "ship," is taken for a new personal name *Ilbum* (no. 58).

We abstain from extending this list and express the hope that future publications by the author may not contain similar errors. His remarks on certain groups of non-Semitic proper names (pp. 15 f.), his suggestions for the reading of the ideograms *URUDU* (p. 27) and *UH-ME* (p. 43), the interesting digressions concerning lines 9 and 10 of the votive inscription from Ashur, *Akkadische Bibliothek* no. VI 2 (p. 22), the construction of the numerals (pp. 59 f.) and (pp. 66 f.) certain permissive forms (where Landsberger, *OLZ* 1925, 230 should have been quoted) show that valuable work may be expected from him.

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*A History of the Expansion of Christianity: Vol. I, The First Five Centuries.* By K. S. LATOURETTE. New York and London: HARPER, 1937. 413 pages, 1 map.

It seems incredible that with all the enthusiasm for missions, Church History, and the study of all features of Christianity, there should not have existed before this anything in English approximating an adequate general treatment of the expansion of Christianity. This need Professor Latourette is attempting to meet in a work which is planned to extend to six volumes, of which this is the first.

A general treatment of the first five centuries of Christianity could not be expected to produce new material, and the first part of the book naturally reads much like a course on Church History.

All it is necessary to say is that it is a good course, well-written, interesting, and thoroughly documented. Necessarily there are many matters treated on which there are differing points of view. While the author has endeavored to avoid controversy and to present all angles of his subject in a scientific manner, in which he is generally successful, he admits that he writes from the viewpoint of a liberal Protestant. There would be little point in trying to find minute mistakes and omissions, but the reviewer may mention that he was unable to find any reference to the Johannine tradition at Ephesus which seems to have superseded the Pauline.

Professor Latourette's distinctive contribution lies in his consideration of the material as indicating the spread of the Church, and in his use of the methods and attitude of ethnologists. Were he himself a trained ethnologist, he would have made even more use of such methods than he has, particularly in the study of the diffusion of specific cultural traits. But in justice to the author it must be said that the application of ethnological methods to historical material is only beginning, and that it was impossible for him to make such detailed studies himself, or to do more than interpret existing matter. Toynbee's recent work (*A Study of History*, 1934) is listed in the bibliography, but the reviewer feels that more use might properly have been made of some of Toynbee's theories and general treatment.

In the section on "Reasons for Ultimate Success," the author recognizes the complexity of his problem, and gives a number of causes without attempting an evaluation of them, but he properly includes as fundamental the influence of the historic Jesus.

The best and most original chapters are those dealing with the effect of Christianity on its environment, and of the effect of the environment on Christianity. Here the author is on controversial ground and moves delicately, especially in such matters as the relation between Christianity and the mystery cults. In general it may be said that he shows good commonsense in his selection, and in those passages where he is forced to summarize and estimate. He takes the position that Christianity did not deliberately borrow from its rivals, but that common conceptions and habits of thought resulted in similarities. One is glad that summaries of such attacks on Christianity as those of Celsus and Prophecy are included, reading often like the criticisms of modernists. It is to be hoped that in the following volumes Professor Latourette will give even more

space to criticisms of Christianity from non-Christian sources, especially Moslem. The effect of Christianity on the status of women is treated too briefly. It is a complex subject, including legal status, the division of labor, participation in social privileges and positions, and actual treatment in everyday life. Nothing is said of the fact that in some directions, the adoption of Christianity resulted in a greater restriction of women than had existed under the pagan empire. The existence of a debased Christianity on the island of Socotra might have been mentioned.

The map is good and not too detailed, and there is an index. The bibliographies are full. In the section dealing with areas outside the Roman Empire, the reviewer suggests the inclusion of W. H. Schoff, *The Periplus of the Erythraean Sea*, New York, Longmans Green, 1912, and L. E. Browne, *The Eclipse of Christianity in Asia*, Cambridge, England, 1933. The latter book might be helpful in later volumes.

The adoption of a foreign religion by a group may be compared to two partly-overlapping circles, in which the resulting culture would be represented by the segment common to both cultures, while the outlying segments represented the cultural features which had been dropped. This is what occurs in the diffusion of religion, or of any other feature of culture. The task of a Christian missionary is to see that the essentials of his religion are included in the new culture, and that features of the old culture which are antagonistic to those essentials are excluded. Such a task involves definition, judgment, and evaluation, which Professor Latourette does not attempt. But his work promises to be the standard treatment of his subject for a considerable time; it fills a long need, and it is a great achievement.

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*Wang An Shih. Vol. II. By H. R. WILLIAMSON. London: PROBSTHAIN, 1937. 424 pages.*

In this volume Dr. Williamson completes his study of the great Sung statesman and reformer, Wang An-shih. It is a sort of mopping-up of various matters that could not be included in the first volume, reviewed in this JOURNAL 56 (1936). 99. Included here are three chapters on the history of the movement after Wang's death; Wang's biography in the Sung dynastic history; sections



dealing in detail with certain of Wang's measures; a general account of the Sung period; accounts of Wang's contributions to literature and translations of twenty-eight of Wang's essays. There are also accounts of two of Wang's predecessors in statesmanship, Sang Hung-yang of the Western Han, and Liu Yen of the T'ang, and a critical estimate of the Sung histories. There are six tables, a Chinese bibliography, and an index.

In the historical chapters, the facts appear to be taken from a book referred to as the *T'ung Chien*, which are interpreted according to Liang Ch'i-ch'iao and Ts'ai Sheng-hsiang. As was pointed out in the review of the first volume, this is not a sufficient identification, as there are a number of works containing these words in their titles, two of which are included in the Chinese bibliography of this volume. The *Ssu k'u ch'ien shu t'i yao* is translated as "The Index to the Imperial Encyclopedia," but might better be rendered "Critical Catalogue of the Ch'ien-lung Library," though neither is an exact translation. The work is more than an index. The *Wen hsien t'ung k'ao* of Ma Tuan-lin is better known in its romanized form than as an "Encyclopedia of Literature," which it is not, being primarily a collection of instances dealing with government. The *Chung kuo jen ming ta ts'u tien*, the *Chung kuo li tai ming jen ch'uan k'ueh*, and Li Ung Ping's *Outlines of Chinese History* (in English), should not be used as sources in a serious study. Nor is Giles' dictionary a final authority.

Many of the notes might be fuller; for instance, the note on Tzu Ch'an (p. 59) does not even give his dates, which are 581-21 B. C. The character 審 (p. 24) should be romanized *shing*, not *sheng*, when used as in the text. The reviewer questions some of the translations of official titles, but did not have the apparatus necessary for making a check. Probably Dr. Williamson did not possess it either, for no authorities are given. It would have been better had Dr. Williamson used hyphens in his romanization of proper names.

It is mentioned (p. 21) that Wang's tablet was placed in the Confucian temple in 1102, next to that of Mencius. There should be a more detailed explanation, for the tablets in the temple are arranged in a number of grades, of which the two highest are *p'ei* (correlator) and *she* (philosopher). The tablet of Mencius was placed on the correlate altar, with those of Yen Hui, Tseng Tzu and Tzu San, in 1084, in spite of opposition because he was not a



contemporary of Confucius. The elevation of Wang An-shih to the rank of correlate, in which he would share the offerings to Confucius, was something unheard of and incredible to Chinese scholars, and in 1126 his tablet was removed from the temple altogether at the instance of Yang Shih.

In the general treatment of the history of the Sung period, it should have been noted that the weakness of the Sung in dealing with foreign powers was largely due to their deliberate reversal of the T'ang policy. The latter gave great power to the frontier governors, so that the foreign policies of the T'ang were generally successful, but the central government was at the mercy of these frontier administrators and generals when they chose to revolt. The Sung reversed this policy, keeping all the power in the central administration, so that they were little troubled with rebellions, but were uniformly unsuccessful in their foreign controversies, because their generals had no power, and were hindered by court intrigues and inefficiency. It might also be noted that a good picture of the military inefficiency and corruption under the Sung may be obtained from a work of fiction, the *Shui Hu*, recently translated by Pearl Buck.

These criticisms are mainly concerned with minor matters, and do not lessen the great contribution which Dr. Williamson has made to western sinology. The two volumes represent an achievement of the first order, giving a detailed picture of a major experiment in the art of government.

*Famous Chinese Plays.* By L. C. ARLINGTON and HAROLD ACTON.  
Peiping: VETCH, 1937. xix + 443 pages; 29 illustrations.

Mr. Arlington is now seventy-eight years old, and since 1879 most of his life has been spent in China, in the service of the Chinese government. He is the author of *The Chinese Theater*, and the reviewer recalls with pleasure hearing him speak on that subject fifteen years ago. Mr. Acton is a young English poet who has lived in Peiping since 1933, and is the author of *Modern Chinese Poetry*.

This volume consists primarily in the translation of thirty-three well-known Chinese plays, the translation being entirely in prose. The illustrations are photographs of Chinese actors in their costumes for parts in these plays. The music of nine arias is given,

transcribed by J. Hope-Johnstone, and there is an index. The introduction gives much information concerning the Chinese theater, including explanations of many technical terms and conventions.

Seven of the plays are concerned with episodes of the Three Kingdoms, one with the medieval period, three with the T'ang, nine with the Sung, ten with the Ming, and two are modern. While there are some notes of an historical nature, and dramatic criticism at the end of each play, there is little or no critical apparatus. Mr. Arlington probably knows more about the Chinese theater than any living westerner, and gives us a wealth of information. The book cannot be called scholarly, but is an excellent popular presentation.

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*Ancient Chinese Bronze Mirrors.* By R. W. SWALLOW. Peiping: HENRI VETOU, 1937. xiv + 78 pages; 105 illustrations.

The chief value of this book is in the illustrations. These give us a handy, although by no means exhaustive series of mirror designs from pre-Han times through the T'ang dynasty, with two illustrations of Sung dynasty reproductions. In general, while the quality of the mirrors shown is often not of the best, the illustrations themselves are good. The mirrors from the collection of M. Henri Lambert grouped together in the latter part of the book seem to be of better quality, on the whole, than those used in the body of the work.

The reviewer understands that Mr. Swallow makes no pretense of learning in literary Chinese, and is but attempting to give us the benefit of his years of experience in collecting mirrors in China. This, no doubt, accounts for the inadequacy of his bibliography, and the general lack of documented footnotes. Under these circumstances this work should be taken in the light of the author's own estimate of it on page xii, viz., "This book makes no claims to be authoritative or final." Hence it would serve no useful purpose to enter into a discussion of controversial questions in the text, or in the translations. In regard to the latter, the inscriptions do not always show up clearly in the photographs, making it difficult to check the author's transcriptions with any degree of accuracy.

What the book really gives us is a popular account of the subject reflecting the author's considerable first-hand acquaintance with both mirrors and the land in which they are found.

There are, however, among others, a number of errors of omission and commission which should be pointed out:

Page 6. *Po Ku T'u* 博古圖 the character 博 does not in itself mean "hundred" as note 4 would imply, but does mean "many" so that a truer translation of the title would be "Diagrams of Many Antiques," rather than "Diagrams of the Hundred Antiques."

Page 7. *Ke* 格 should be read *Ko*.

Page 9. *Lieh Kuo* 列國 does not mean "Warring States," the usual designation for that period being *Chao Kuo* 戰國. This should also be noted in the index, page 75.

Page 13. The fourth character *t'ung* 同 in the transcription at the foot of the page is written correctly 銅 on the mirror.

Page 29. The last character of the inscription on the mirror shown opposite is omitted from the transcription. The character is *ju* 如.

Page 45. I agree with the author that the name "Sunlight Mirror" as indicating a type is not a happy one. The Chinese catalogues do not use the designation in that sense, but use it merely to identify particular mirrors whose inscriptions begin with the two characters *sh kuang* 日光. Other mirrors of the same type but with different inscriptions are designated by the first two characters of their inscriptions, whatever they may be. On the other hand, the author makes a mistake in suggesting "*pi mu ching*" 比目鏡 or "Double Bird Mirror" as an alternative name. The characters *pi mu* 比目 refer to fish and not to birds, although a counter allusion is possible; no doubt he is thinking of *pi i niao* 比翼鳥, but it is not entirely clear that the design represents birds.

Illustration 113 of M. Lambert's collection, opposite page 61, bears a different inscription from that transcribed below it as belonging to it.

In addition to the above, there are such mistakes in proof reading as on page 21, line 2 of the translation at the foot of the page, "a geni" instead of "a genius," and page 29, second paragraph, "ornated" for "ornamented." On the illustrations, in many cases, no indication as to the ownership of the object is given.

Appendix 2 by Mr. S. J. E. Jensen gives interesting chemical analyses of seven pieces of ancient Chinese bronze and compares them with an ancient Egyptian mirror and two modern telescope reflectors. With no intent to detract from Mr. Jensen's work which is valuable as far as it goes, the reviewer would like to point out that no sweeping deductions as to the composition of ancient Chinese bronzes, or the skill of the ancient Chinese in refining metal, should be drawn from so small a series. "A Chemical Study

of Ancient Chinese Bronzes" by S. Komatsu and Y. Yamaguchi in *Tôhō Gakuhô*, no. 3, March 1933, gives analyses of thirty-five ancient Chinese bronze mirrors and finds no two alike in composition.

Mr. Swallow's book is well printed and contains a useful glossary and an index.

A. G. WENLEY.

Freer Gallery of Art,  
Washington.

*An Introduction to Zen Buddhism.* By DAISETZ TEITARO SUZUKI. Kyoto: EASTERN BUDDHIST SOCIETY, 1934. 130 pp.

*The Training of the Zen Buddhist Monk.* By DAISETZ TEITARO SUZUKI. With illustrations by Zenshu Sato. Kyoto: EASTERN BUDDHIST SOCIETY, 1934. xiv + 111 pp., 43 plates and illus.

*Manual of Zen Buddhism.* By DAISETZ TEITARO SUZUKI. The Ataka Buddhist Library VIII. Kyoto: EASTERN BUDDHIST SOCIETY, 1935. x + 232 pp., plates, illus.

[American agents for these volumes: MARSHALL JONES CO., Boston.]

To the question, "What is Zen?" Dr. Suzuki replies that Zen is the whole mind. There are in Zen no sacred books or dogmatic tenets, nor are there any symbolic formulae through which an access might be gained into the significance. Zen teaches nothing, and whatever teachings there are come out of one's mind, and Zen merely points the way. There is no God in Zen. It states, "the immaculate Yogins do not enter Nirvana and the precept-violating monks do not go to hell." Zen is the spirit of a man, and believes in his inner purity and goodness.

Rising out of the Far Eastern Buddhism, Zen claims to transmit the essence and spirit of Buddhism directly from its author, and not through any documents or rites. Personal experience is everything in Zen. On the other hand common-sense way of looking at things is not final; Zen insists that we acquire a new point of view, since the ordinary logical process of reasoning is powerless to give final satisfaction to our deepest spiritual needs. By constant negation of the logical system upon which we have been nourished, we attain a higher affirmation where negation and assertion are unified, and this is called the essence of Zen.

Zen is practical; hence it never wastes time or words in explanation. Dr. Suzuki writes: "A gong is struck and its vibrations instantly follow; if we are not on the alert, we fail to catch them." The ultimate goal of Zen is an intuitive understanding in contrast to intellectual and logical understanding, and it is obtained through meditation and discipline, which are fully explained in the *Training of the Zen Buddhist Monk*.

In this first volume, Dr. Suzuki gives enough illustrations from the sayings of great Zen masters to explain the baffling problems connected with the doctrine, and yet often the real significance is hard to grasp. As he repeatedly states, the essence of Zen cannot be got through explanation, and yet for an average Westerner a sentence or two here and there pointing to the correct interpretation might help much to facilitate his understanding and spiritual attainment. Nevertheless, the volume is significant since this is the first book that ever attempted to explain the mystery of Zen to a layman.

After reading through Dr. Suzuki's *Introduction to Zen Buddhism*, one naturally wonders what sort of training a Zen monk goes through in order to attain that spiritual insight which has been referred to as its foundation. In its beginning, there was no specified method; a master would communicate his truth by striking a novice with a stick, slapping him in the face, or kicking him. Through incoherent ejaculation or a hearty laughter, or even with abusive remarks, he endeavored to enlighten his truth-seeking disciple. With the development of the *koan* exercise in the 11th century, the understanding of Zen came to be much easier. *Koan*, literally meaning "official document," is a kind of problem given to students for solution which leads to the realization of the truth.

The initial training of a Zen monk takes place at the Seat of Perfect Wisdom, built for the purpose. After his initiation to the Brotherhood, at once his life of humility begins with begging, which beside its economic value, has a two-fold moral significance: first to teach the beggar humility and the second to make the donor accumulate the merit of self-denial. A motto, "A day of no work is a day of no eating," is the rule of Zen discipline. He is engaged in all kinds of manual labor and the monastery is self-supporting. Through the life of service, he learns to make animate and inanimate objects live. The saying "to make a living use of a thing"

means to develop its efficiency to the highest possible degree. Through the life of labor and service and humility, the monk now attains the life of prayer and gratitude. In Zen, prayers are offered to all the Buddhas and Bodhisattvas of all time, and they are more in the form of self-reflection and vows than of asking for help in accomplishing desires. Finally he comes to the life of meditation which is the central fact of the Zendo life, where the claims of the body are minimized in order to divert their course to a higher realm. Study of the old masters and their sayings, a tea ceremony, and discourse with fellow students are the routine of their daily life, and upon the completion of the term a novice is now ready to go into the world.

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With the two previous volumes, the *Manual* completes a triptych on Zen Buddhism, and its object is to inform the readers of the various literary materials relating to the monastery life, what the Zen monks read in their daily services, where their thoughts move, and what objects of worship they have in the different quarters of their institution.

*Gatha*, verse or hymn, is the versified portion of the sutra, and I will quote the *Four Great Vows* which are recited after every service:

However innumerable beings are, I vow to save them;  
 However inexhaustible the passions are, I vow to extinguish them;  
 However immeasurable the Dharmas are, I vow to master them;  
 However incomparable the Buddha-truth, I vow to attain it.

The sutras most read in Zen are *Prajñāpāramitāhṛdaya*, *Samantabuddhapariśuṭa*, and *Vajracchedikā*. These together with the two others from the Mahayana school are translated by Dr. Suzuki.

The most interesting part of the *Manual* is that which is devoted to a selection of literature from the Chinese and Japanese Zen masters. At first there was not much to distinguish Zen specifically from the rest of Buddhist literature, but in the course of time, there grew up what is now known as the *Fū-fu*. As Dr. Suzuki points out, one of the chief characteristics of the Zen *Fū-fu* is the free use of colloquial expressions which are not found in the classical literature. The two versions of the famous *Ten Ozherding Pictures* are reproduced with the translated texts.

The last section on Buddhist statues and pictures in the Zen monastery is very useful to students of Japanese art and literature.

*Katami Karanigama. Lectures, Essays and Studies.* By MASAHARU ANESAKI. Boston: MARSHALL JONES, 1936. vi + 318 pp.

The essays in the present volume by Dr. Anesaki were collected by his former pupils at the occasion of his sixty-first birthday anniversary from various journals and presented to him. Dr. Anesaki chose the title, *Katami karaniyama, akāṭṭa bhārā*—"Done what is to be done, burdens released"—a passage in the Pāli Canon describing the attainment of the Arahant, the ideal perfection of Buddhist training. He writes:

Having done what was to be done so far, what does remain for me to do in the second stage of my life? I feel there is something still. Perhaps my life up to the present has been only a preliminary stage to the real work to be done in the future, not only in this life of mine but in lives to come, whether "mine" or "ours."

As he was the first Japanese scholar to delve into the science of religion, so the first group of essays is in the field of comparative religion. In "How Christianity Appeals to a Japanese Buddhist," Dr. Anesaki finds that the difference between the two is the intellectual character of Buddhism and the emotional character of Christianity. The former is the religion of resignation while the latter that of hope, love, and faith. He concludes:

Where there is the faith in Buddha, there may grow the faith in Christ. . . . Japan may remain Buddhist or be converted to Christianity, but she will in either way keep her own tone of national spirit and civilization, and in this way play a part in the grand concert of humanity.

The second group of essays centers around the themes of social and religious problems of Japan. The religious history of Japan is characterized by incessant influxes of foreign religions, and the minds of her people have not remained wholly passive towards these influences. Dr. Anesaki traces the various struggles and conflicting influences, and writes, "Christianity in Japan, no less than Buddhism and Shinto, is in a critical situation, a period of transition or reformation," but "there is light in the midst of conflicts."

The essay entitled "Social Unrest and Spiritual Agitation in Present-day Japan" was written in 1922, but after fifteen years the author's analysis of the problems remains still valid. The ferment, he finds, is not limited to the political and social *ufernā*, but "goes deep down to the very roots of human life, and the turbulence manifests itself in demands for wanton emancipation of the instincts

of human nature." He considers the whole matter as a conflict between the instinct and the reason and a curious combination of fear and pride. He is optimistic and finds the light in such men as Toyohiko Kagawa and Tenko Nishida.

The last section of the book devoted to Buddhistic themes is perhaps the most interesting to Western readers, but since the articles here reprinted are in the *Encyclopaedia of Religion and Ethics*, I shall not discuss them. "Some Problems of the Textual History of the Buddhist Scriptures," written in 1907, brings to light what the translators of the Agama texts into Chinese have done by their painstaking labours up to the end of the 5th century. Since then the textual criticism of the Buddhist sacred writings has made remarkable progress.

Since many of the essays in the present volume are the author's public lectures delivered in various parts of the world, they naturally lack the high quality of scholarship and stylistic beauty which characterized his other works, but taken as a whole, there is a definite continuity and integrity. The author's solution of Japan's problems and her ills may not be correct, but an intelligent reader should glean new light on Japan.

A complete bibliography of the author's writings is appended.

SEIO SAKANISHI

Library of Congress.

*Comptes Rendus de Onze Années (1923-1933) de Séjour et D'Exploration dans le Bassin du Fleuve Jaune, du Pei Ho, et des Autres Tributaires du Golfe du Pei Tcheu ly.* By EMILE LICENT, S. J. (Publications du Musée Hoang Ho-Pai Ho, No. 38.) Three volumes, 1061 pages, an index, and a loose-leaf atlas of 77 maps. Limited edition of 600 copies. The subscription price in U. S. A. is \$45.

For the last two decades the most significant addition to our knowledge of geology, archaeology, botany, and zoology of the uncharted regions in China and Asia is the results of the work and energy of Father Emile Licent. Founder and director of Huan Ho-Pai Ho Museum in Tientsin, Father Licent has spent more than two decades of exploration and study in the Far East.

The results of his investigations during 1914-1923 in the region of Kansu and Kokonor, were published in 1925 under the title:



"Dix Années" (Ten Years' Investigations). The present work, which covers the period of 1923-1933, is a continuation of the first monograph, and embodies the day-by-day account of his work of exploration in the basin of the Yellow River, Pai Ho, and the tributaries flowing into the Gulf of Chili.

This work consists of three volumes of text, with over 1250 pages and numerous text illustrations, the Key map and a loose-leaf folio of 77 separate maps in 1.67 m. to an inch scale, and a complete travel survey by the author, supplemented by 1370 photographs of the most important points on the route. An index of places and names, given in French and Chinese characters, a table of stop-over places with cross-references to the maps and the text, a list of illustrations, similarly cross-indexed, and a detailed index and bibliography, add greatly to the facility of using this monumental work.

To students of archaeology this work is of great importance, as it includes the exploration of the Mission Paléontologique Française (Licent and Teilhard de Chardin 1923-24 and 1926-27), which has discovered in Chouai-Tong-Hou, Sjaru-Osson Gol, and in the base of the Great Loess, the First Palaeolithic sites in China. Not less important, for the palaeontologist, is that portion of the work which deals with the famous discovery of the fossil fauna of Sanmenian type in the region of Ni-ho-wan.

Covering parts of Manchuria, Inner Mongolia, Ordos, Shensi, Shansi, Hopei, Honan, and Shantung regions, this travelogue provides innumerable data on the geology, botany, zoölogy, prehistory, climate, geography, history, economics, and ethnography of the region.

Father Licent describes his work with Teilhard de Chardin, geologist and palaeontologist, on Sanmenian fauna; with H. Serre, C. M., botanist, collecting a huge herbarium in the hills of Hsuan-Huanfu; with P. Leroy, working on the marine life along the coast of Shantung; and with M. Trassaert studying the insects of K'o Lan Shun.

The study of the fossils in the Ordos desert and Northern Shansi, the palaeolithic sites of Chou Tong Keou, the neolithic remains in Kalgan, the fossil beds of Sang Kar Ho and Chou K'ou T'ien, the volcanoes of North Shansi, the marine life of Shantung, and the fauna and flora of the mountains in Central and Northern Shansi are just the high lights of his numerous achievements.

This enormous diary, so scrupulously kept, day-by-day, forms the background and source of original data, for a number of scientific monographs dealing with the various aspects of his journeys. Thus, the two seasons' work on the neolithic sites in the region of Koa-Kia Yiang Tze, northeast of Kalgan, are published under the title: "Les collection Néolithique du Musée Hoang Ho-Pai Ho." (No. 14, 1932, of the Publications of the H. H. P. H. Museum.) V. Pavlov describes the reptiles collected in the region of Ordoz in Vol. 23 of the same series: "Reptilia et Amphibia du Musée H. H. P. H. avec un note sur le genre *Phrynoscephalus* par E. Licent." The famous palaeolithic discoveries are dealt with in such publications as: "Le Paléolithique de la Chine," E. Licent et P. Teilhard de Chardin, in Vol. XXXV of *L'Anthropologie*, or the monograph of the same title in *Archives de L'Institut de Paléontologie Humaine*, written in collaboration with M. Boule, H. Breuill, and Teilhard de Chardin. The find of a human tooth was dealt with in the article: "On the Presumably Pleistocene Human Teeth from Sjura-Osson Gol" (*Bull. Geol. Soc. China*, Vol. V. No. 3-4) written with D. Black and Teilhard de Chardin.

To review this book adequately is to give it in full detail. This being impossible, the next best is to recommend it to students of the Far East. This much can be done without any reservations.

EUGENE A. GOLOMANROK.

The University Museum,  
Philadelphia.

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